NATIONAL ACADEMY OF SCIENCES
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OF

JOSEPH BARRELL
1869-1919

BY

CHARLES SCHUCHERT

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1925

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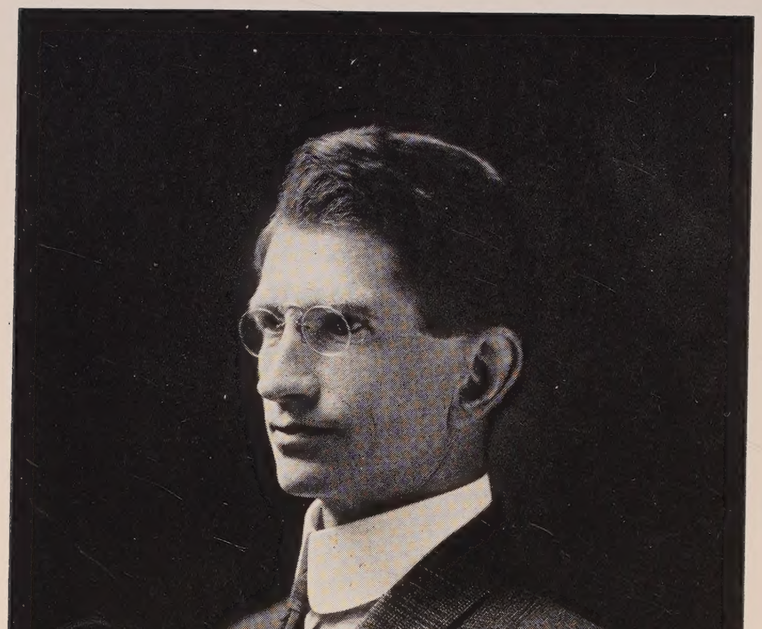

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JOSEPH BARRELL, ENGINEER-GEOLOGIST

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JOSEPH BARRELL'S scientific life coincided with the "Golden Era" of Geology in America, and in him American Geology has lost a leader who promised to stand as high as the highest. "Those whom the gods love, die young." His period of educational preparation, balancing of personal characteristics, and storing up of fundamental experiences was back of him. Had he lived longer he would have become the chief exponent in the subjects of geologic sedimentation, metamorphism, structural geology, the geologic bearings of isostasy, and the genesis of the earth. T. C. Chamberlin writes: "We had come to look upon him as one of the most promising leaders in the deeper problems of earth science"; Bailey Willis, that "there is unanimous recognition of the fact that Barrell was one of the strongest of the younger leaders and a man of great promise"; John M. Clarke, that Barrell's death "is a truly overwhelming disaster for American Geology."

Upon his colleagues at Yale, Barrell's death, following so soon after that of Irving, fell as a heavy blow. Coming to us as a matured and highly educated young man, we saw Barrell grow into a leading geologist who exceeded our hopes and more than justified our choice of him to fill the chair of Structural Geology created for him at Yale. He was a power among us, and it was around him that our graduate courses in Geology were built. Personally, we are bereft of a friend and councillor whose place can not be filled—one whose simplicity of nature and strength of character were unique. His great fund of knowledge was always gladly placed at our disposal, and his constructive criticism and fertility of suggestion have been the stimulus to more of our work than we shall ever realize. We rejoice that the privilege was ours of working with him, and in our hearts there will always remain the grateful memory of his inspiring personality.

Barrell's death occurred in New Haven, Connecticut, on May 4, 1919, after a week's illness with pneumonia and spinal meningitis. He left a wife, Lena Hopper Bailey, and four sons, Joseph, Herbert Bailey, William Colburn,* and Richard Lull. Standing 5 feet 10.5 inches in height, of the blue-eyed Nordic type, with a full head of wavy light brown hair, he was pale and spare of build, and yet of great muscular strength—the "strong man" of his class at Lehigh. Once seen, he was easily remembered, and he was quickly picked out in a crowd. This was due in part to his tall slender build, his long and awkward stride, and his confident bearing, but more especially to the strength of character reflected in his large features, particularly the wide mouth and long, narrow nose, concave in profile. He prided himself on the longevity of his ancestors, and from a careful study of insurance tables held the unshakable belief that some thirty years of active life were ahead of him.

Modest and optimistic, with a strong independence of mind, he prized true worth highly, and was easily aroused to criticism of posers and social climbers, and of shams and errors. Simple in attire, and fond of simple living, his intellectual ideals were of the highest. He cared little for popularity, or for adverse criticism, and not much more for praise. His colleague, Professor Gregory, says that what he valued most was "uninterrupted time for research and intellectual fellowship established through writings. His intellectual power was so obvious and so continuously displayed that twenty years of intimacy has left on me an impression of a mind rather than of a man. His mind was of surpassing fertility, imagination, and machine-logic." In appearance and mentality, Barrell reminded his older colleagues at Yale of James D. Dana; whatever subject these two great geologists touched was made clearer and the way indicated for new lines of research.

A man of science, and especially one deeply interested in generalizations, should be endowed with imagination under restraint. Barrell had a great deal of this quality, and loved to speculate under the limitations of the "multiple hypothesis." No man ever had better developed the power of detachment

* Since deceased.

from his own views. He could examine his conclusions from all angles. As Davis says, "He interested himself in thinking about how he thought, and tried to evaluate the results of his thinking. He was as careful and critical in this respect as he was fertile and ingenious in mental inventions."

An omnivorous reader and a very hard worker, Barrell never tired of unraveling the intricacies of earth structure, and having a marked faculty for picturing his thoughts, he not infrequently expressed an idea in graphic form before he put it in writing. His method of presenting a subject was to prepare his reader for what was to come, and then to set forth in detail the processes and principles that underlie the results sought for. This is why most of his papers are lengthy and in places tedious. His earlier articles were not well written, and he constantly sought to improve this weakness.

In the scheme of instruction at Yale, Barrell had charge of two classes: a half-year undergraduate course in Historical Geology, and a full-year course, primarily for graduates, in Dynamical and Structural Geology. At times he also took part in teaching small groups in Field Geology, and nearly every year he gave a special course to graduate students in which the literature and the solution of problems along structural and dynamic lines constituted the subject-matter. Before the class, Gregory says, "he was the geologist rather than the teacher of untrained minds. . . . The recognition of the average undergraduate viewpoint played little part in his teaching." Accordingly, in his earlier career at Yale the boys found him too statistical, and on the campus he was known as "Old Bathylith" and "Interstitial Relations"; later, however, these friendly nicknames vanished.

In graduate teaching, Gregory continues, "where analytical thought, mental invention, breadth and depth of knowledge make the successful instructor, Barrell had few equals." The more mature his students, the more enthusiastic they were in his praise, and he gave them much that was not to be found in books. Difficult his course was, and exacting his examinations, but the student who completed them was thoroughly grounded in the principles of Dynamical and Structural Geology.

FAMILY HISTORY [1]

The name Barrell, spelled in many ways, had its origin among the ancient land-holding knights of Normandy, and appears in England for the first time with the battle of Hastings and William the Conqueror in 1066. The first of the family to migrate to America was George Barrell, a cooper by trade, who arrived at Boston from St. Michaels, Suffolk, in 1637, and died there in 1643. He became a freeman of the Boston parish on May 10, 1643, and owned a house and a half acre of ground (for which he paid 31 pounds) on what is now the southeast side of Hanover Street between Elm and Washington. He had but two sons, and one of them, John, also a cooper by trade, married Mary, daughter of William Colburn, one of the twelve original founders of the colony. It is from this union that all of the American Barrells of colonial origin have sprung.

Until recently, the Barrells have been in the main sea-going people, ship-owners and merchants. The second John Barrell was a mariner, and his son John a well educated man and a successful shipping merchant. Professor Barrell in his genealogy of the family says of John III: "The hazards of travel and of residence in tropical lands, however, told severely upon their number, so that notwithstanding several large families of sons, his descendants bearing the name have remained few in number and widely scattered."

The most widely known and wealthiest was Joseph Barrell of Boston (1739/40-1804), after whom the subject of our sketch, his great grandson, was named. He married three times and had twenty children. This Joseph Barrell was an original thinker and a good speaker and writer. He is said to have "early espoused and firmly maintained the cause of his country," and for a time represented the town of Boston in the State Legislature. He lived well, and it was in his splendid home that General Washington was entertained during his visit to Boston. He was also one of the group of men who fitted out the ship "Columbia" and sent her into the Pacific,

[1] Taken from a genealogy of the Barrell family prepared by Professor Barrell, but not published.

where in 1792 her crew discovered the Columbia River. Later they purchased of the Indians the territory about this stream, and in this way began the colonization of what have since grown to be the Northern Pacific States of the American Union.

The father of Professor Barrell, Henry Ferdinand, was born in New York City, October 3, 1833. His son says he "grew up with a strongly developed taste for books, for nature, and for life in the country." Henry Ferdinand's father bought him a farm near Warwick, Orange County, New York, and it was here that he met his wife, Elizabeth Wisner, whom he married on March 15, 1858. The Wisners, originally from Switzerland, had been real estate holders for 150 years and officers in the colonial and later wars. In 1864, Henry Ferdinand Barrell sold this farm and bought another at New Providence, New Jersey, and here from 1875 to 1895 he served as chairman of the trustees of the public library and of the public school in which the subject of this sketch received his primary education. He had nine children, of whom Joseph was the fifth child and the fourth son.

ACADEMIC CAREER

Joseph Barrell, the subject of this memoir, was born at New Providence on December 15, 1869. As a child, he was more interested in books on natural history, astronomy, and history, than on general literature. His mother, after whom he takes, related to the writer that frequently he would get a volume of the Encyclopedia Brittanica and sit for hours reading it. At the age of ten years, he was given a planisphere and often at night would take it and a book on astronomy, along with a lantern, and then lie on his back gazing at the stars, and so learn their names with the aid of the planisphere. Joseph attended the public school until he was sixteen years old, and two of his teachers prepared him privately for college. Before going to college, however, it was necessary for him to earn some money, and so he taught school during 1886-1887. For this he received two hundred dollars, and with further assistance from his parents he attended the Stevens Preparatory School at Hoboken the following year. Here he won a scholarship for Stevens Institute, but preferred a college course at

Lehigh University, which he began in September, 1888, and completed four years later with high honors. In 1893 he received from the same university the E. M. degree, in 1897 its M. S., and in 1916 its doctorate of science.

In a sketch of himself written for the twenty-fifth anniversary of his graduation at Lehigh, Barrell says that in 1893, when he received his engineering degree, "I regarded myself as lucky in securing an instructorship at Lehigh in mining and metallurgy." This position he held for four years, teaching mechanical drawing, mining and metallurgical design, making shop visits to various metallurgical plants, and practising surveying with students in the anthracite mines. "Teaching," he goes on to say, "is always better training for the teacher than for the taught. . . . The summers were put in in gaining experience, parts of the winters were employed in studying Geology and practical Astronomy, for which Lehigh gave me the degree of M. S. in 1897." His thesis in fulfillment of the requirements for this degree is 419 pages long, and is entitled "The Geological History of the Archean Highlands of New Jersey, including their Extension in New York and Pennsylvania." In it appear some of the problems on which he later worked so much.

In 1898, Professor E. H. Williams, Jr., contemplating a division of his work at Lehigh, persuaded the university to hold vacant for two years the position of assistant professor of Geology, provided Barrell would spend that time at Yale in advanced work. Barrell says that this opportunity "was a most generous one. I spent the following two collegiate years at Yale and the summers working in Montana for the U. S. Geological Survey in general and mining Geology." In 1900 Yale gave him the degree of Ph. D., and tradition has it that he was the ablest student of Professors Penfield, Pirsson, and Beecher. In this way, as Barrell says, "a mining and metallurgical education, combined with a panic year on leaving college, had led logically into a career as a geologist. The initial engineering education and the experience of the eight years following 1892 formed the broad and solid base on which the following work has been built."

For three years after 1900, Barrell taught Geology at Lehigh,

with Zoology as a side issue. In 1902, he was married there to Lena Hopper Bailey. The three summer months of the year preceding were spent in Europe with Professors Herbert E. Gregory and Charles H. Warren, travelling "by foot, by bicycle, and by third-class trains, the object being to see the countries and study Geology rather than to do sightseeing in the cities."

"What better course," says Gregory, "could be devised for a man set aside as a physical geologist than that chosen for and by Barrell: elementary school and high school in a rural village, a year at a city preparatory school, four years of engineering studies, six years of mining practice combined with teaching of mathematics, astronomy, mining and metallurgy, and capped by two years of graduate study of field and laboratory problems? Few men at the age of thirty-one have built such a broad and solid base for a future scholarly career."

In 1903, Barrell was asked to come to Yale to develop the field of Structural Geology, and his decision to accept the invitation was the turning point in his career, since by it he turned his back on the more profitable field of mining engineering and set his face toward graduate teaching and research. He was at first assigned to the Sheffield Scientific School, but Professor Gregory's classes in Geology in Yale College had grown so rapidly that Barrell was transferred to that department of the University in the following year. Needing still more help, the College in 1905 appointed Isaiah Bowman to teach Physiography and Geology, and two years later Ellsworth Huntington came to give work in Geography. All of this teaching was done in the Peabody Museum, where E. S. Dana, Charles Schuchert, and R. S. Lull also had their offices. This group of men reacted intellectually on one another in their daily intercourse, and here the extent of Barrell's dynamic influence can not be measured, except as the printed product of the other men in the decade between 1905 and 1915 bears testimony to it.

Barrell was a member of the honorary scientific society of the Sigma Xi, and president of the Yale Chapter in 1914. He was also elected to the Phi Beta Kappa Chapter of the same University. He was a fellow of the Geological Society of America, the Paleontological Society, and the American Asso-

ciation for the Advancement of Science, and a member of the American Academy of Arts and Sciences. Only four days before his death, there came to him the news of the highest honor that can be given to an American scientist, election to the National Academy of Sciences.

As a lecturer, Barrell was often called on by other universities. In 1912 he gave a series of five lectures at the University of Illinois, dealing with "The Bearing of Geology on Man's Place in Nature" and "The Measurements of Geologic Time." In 1914 he gave a course of three Sigma Xi lectures at the Universities of Missouri and Kansas. At Columbia two years later he gave six lectures on isostasy, and at Yale he presented before the Sigma Xi and Phi Beta Kappa societies his interesting talk on "The Habitability of Worlds."

WRITINGS

An analysis of Barrell's writings shows that he progressed from simpler relations to the most complex of geologic problems. It is also clear that his best results were obtained through generalizing from the publications of others. He loved to assemble the published data derived from field and laboratory, and along with his own observations, subject all to the test of multiple hypotheses, so as all the better to ascertain the correct explanation of the facts examined. As Willis has said, Barrell was first an engineer in Geology, since his training had led him to precise habits of thought, and it is this characteristic that especially distinguishes his work. He himself has said that "geologic research in the past generation has been passing out of the qualitative stage and has partaken notably of the quantitative character."

Barrell's first publications, in 1899 and 1900, deal in mining, but those since 1901 have nearly all had to do with Geology. His bibliography, if completed in detail, would take note of about 150 notices and reviews of books, most of which appeared in the *American Journal of Science*. Of these, nineteen contain original matter and are therefore included in the bibliography which accompanies this sketch; in general they deal with isostasy, the origin of the earth, and metamorphism, the subjects

with which he was most familiar. When he set himself to write a review of a book, he produced a lucid analysis, with discussions of the conclusion attained by the author.

Of short papers and longer memoirs, his bibliography includes about sixty, totalling nearly 2000 pages.

MINING ENGINEERING

Barrell's first experience as a mining engineer was in 1894, with the engineering corps of the Lehigh Valley Coal Company, at Wilkesbarre, Pennsylvania. In June, 1897, he joined the staff of the Butte and Boston Mining Company of Butte, Montana, and worked with them and the Boston and Montana Company for over a year. "The work was interesting," he says, "and involved difficult problems in the plumbing of crooked shafts, and the measurements of amounts of ore extracted from old workings." These experiences led to his publishing in *Mines and Minerals* during 1899 and 1900 a series of five papers which he wrote while at Yale studying for the doctor's degree. These papers have to do with the methods and errors of mine surveying,—instrumental errors, methods of keeping stope books, and choice of survey instruments. They abound in mathematics and in diagrams, and thus foreshadow two of Barrell's future tendencies in Geology.

REGIONAL GEOLOGY AND METAMORPHISM

During the summer months of 1899, Barrell was field assistant to W. H. Weed of the United States Geological Survey, mapping the ore-bearing formations of the Elkhorn Mining District of Montana. This work, Barrell states, was done alone, much of it on horseback, in the mountainous region between Butte and Helena. It involved a study of the great successive intrusions of molten rock which in the early Tertiary had broken up the crust and brought in the wealth of gold, silver, and copper. The first result of these field studies was the publication of the "Geology and Ore Deposits of the Elkhorn Mining District," by W. H. Weed, with an "Appendix on the Microscopical Petrography of the District," by Barrell.

These observations Barrell made the basis of a dissertation for the doctorate when he returned to Yale in the autumn of 1899. Under the guidance of Professor Pirsson, he made an elaborate petrographic study of the rocks, along with the geology of the Elkhorn area, resulting in a dissertation which was entitled "The Geology of the Elkhorn Mining District."

This Elkhorn work contained a chapter on "The Physical Effects of Contact Metamorphism," which was published in abstract in the *American Journal of Science* in 1902. In this paper Barrell discussed the changes of mass and volume through metamorphism, and stated, among other things, that the shrinkage in rock of certain compositions may be "from 25 to 50 per cent in volume, attended with the evolution of great quantities of gases which at surface pressures and temperatures would amount to several hundred times the volume of the original sediments."

In the summer of 1900, Barrell was again employed by the United States Geological Survey in a two months' reconnaissance of the surface geology of the Deerlodge region of Montana, and of the underground geology of Butte. The next year he began, again under the direction of Weed, a three months' geological survey of the surface and underground geology of the Marysville mining district, Montana. These results, also worked out at Yale and published as "Geology of the Marysville Mining District, Montana: a Study of Igneous Intrusions and Contact Metamorphism," reveal a mastery of petrography and chemical mineralogy. The region was one of the noted gold-producing centers of Montana, and the mines were situated around the margins of the irregular Marysville bathylith of quartz diorite. The Marysville bathylith is "but 6 miles at its nearest point from the exposed surface of the far greater Boulder bathylith, a granitic mass which is petrographically a quartz monzonite in normal composition. The Boulder bathylith possesses a general rudely rectangular form, occupying about 60 miles in latitude by about 35 in longitude, and holds within its confines the mining city of Butte, from which for many years past has poured a flood of silver and a quarter of the world's copper. Other smaller mining centers also lie within this large granitic area, while such important ore deposits as

those of Elkhorn and Unionville, south of Helena, have been found about its margin."

The Marysville report has now become one of the classics in Geology, and of it Pirsson in a review in the *American Journal of Science* says: "The special character of this work lies in the detailed investigation of the bathylithic body, of the method of intrusions, of its form, and of its relations to the surrounding rock masses both past and present." The intrusion Barrell could not explain by the accepted methods, and did so by a new theory, that of magmatic stoping. Daly, in his book "Igneous Rocks and their Origin," states that "It was in the Cordilleran region, at Marysville, Montana, that Barrell independently originated the stoping theory of magmatic emplacement."

Because of the large scale on which the Boulder and Marysville bathyliths are exposed, and because of the forceful presentation of the field relations and the clearness of Barrell's inferences therefrom, Suess, in his great work, "The Face of the Earth" was led to say that the Marysville report is "one of the most instructive works produced in modern times" connecting granitic invasions with volcanoes.

In 1904 appeared Van Hise's epochal "Treatise on Metamorphism." Great in volume and depth of thought, this book made a profound impression on Barrell, and his copy of it was read and re-read so often that it soon fell to pieces. All of Barrell's students also had to do much reading in this book, and in 1905 he wrote an extended review of it for the *American Journal of Science,* in which he states that Van Hise has made of metamorphism "an exact science," that its phenomena express the "chemical and physical laws operating within the crust of the earth," and that they are here presented "in the most systematized form." The book is a "landmark of a new era." To Barrell, however, Chapter 11 was the most interesting, since it treats of the relations of metamorphism to the distribution of the chemical elements, and in his notice he devotes more than one half of the space to it.

Metamorphism always remained one of Barrell's foremost lines of study, and as late as 1914 he wrote "Relations of Subjacent Igneous Invasions to Regional Metamorphism."

This paper was published in 1921 under the editorship of Professor Frank F. Grout. Some of its summations are as follows: Bathyliths come into place either accompanied by crustal compression, as in the Appalachians, or without folding, as in the Cordilleras. The features of metamorphic rocks are due in the main to bathylithic invasion and compression; to a less extent to movements of solutions, selective crystallization, lit-par-lit injection gneisses, and the alternation of injection and mashing. Magmatic solutions are not of meteoric origin, and the results in minerals depend upon equilibria,—largely on the presence of H_2O and CO_2.

EROSION, SEDIMENTATION, AND CLIMATOLOGY

After some years as an instructor of graduate students in Dynamic Geology, Barrell's ideas in regard to processes of erosion, sedimentation, the formation of deltas, and the discerning of ancient climates in the sediments took form, and it was on these subjects that he next wrote. He it was, more than any other, who led geologists away from the prevalent idea that nearly all strata are of marine origin, and toward what Coleman has called "dry land geology," namely, to the recognition that one fifth of the present lands are mantled by continental deposits. Nearly one fourth of his publications follow these lines. As Vaughan says, Barrell organized the principles of sedimentation "into a consistent body of usable criteria, and applied them to the interpretation of many formations." He also did much to build up the science of Paleoclimatology, and in Paleogeography he established principles for discerning the shore-lines of the seas, and the extent and elevation of the ancient lands that were furnishing the marine sediments. Had he lived longer, he would have done much more, for an unusually stimulating opportunity had come to him as chairman of the newly founded Committee on Sedimentation in the National Research Council.

In 1906, Barrell published a series of three papers on the "Relative Geological Importance of Continental, Littoral and Marine Sedimentation" in the *Journal of Geology*. Here, as in most of his papers on sedimentation, he is dealing with fresh-water deposits, those of deltas, and those of the littoral

region of the seas. To have a correct knowledge of marine sediments one should of course know their origin and nature on the lands, and while this logical sequence for study had much to do in leading him to work mainly with continental formations, the fact of his environment probably had most to do with these predilections. Barrell was raised on Triassic strata that are composed of conglomerates of fresh-water origin, and as a teacher of students at Yale his excursions were largely to fresh-water and estuarine strata. James D. Dana had long been explaining the Triassic strata in eastern North America as of estuarine origin; to-day everyone sees that their genesis came through fresh water in areas between the mountains of Permian making, and under semiarid climates.

In these three papers, Barrell sets forth a quantitative view as to the relative geologic importance of continental, littoral, and marine types of sediments, and the criteria for separating them. It is a study of facts already assembled in the geologic literature, and an application of changing and cyclic geographies to stratigraphy. Among other things he develops the criteria for discerning subaerial delta deposits, and shows that such attain their greatest development after epochs of mountain making unaccompanied by notable uplift of the continental platforms. "The ratio of continental to marine sediments," he says, "should have fluctuated widely through geological time. Following an epoch of continental uplift with mountain-making, the deposits formed in interior basins should attain a maximum, especially the deposits made under desert conditions." Finally, when the lands are greatly reduced, the burden of the rivers is largely that of solution materials, resulting in the wide spread of shallow-water marine deposits.

He also emphasizes the cyclic relations between continental and marine sedimentation in geologic history. The wide and cyclic significance of mud-cracks in association with other features indicating flood-plain deposits is discussed at length and applied to the interpretation of Proterozoic deposits in Montana and the Grand Canyon of the Colorado.

The significance of desert deposits becomes very striking when one notes that one fifth of the present land surface is desert tracts. Barrell estimates that the subaerial deposits

of piedmont waste, of continental basins, and of deltas cover about one tenth of the emerged continental surfaces. Adding these "to the estimate of the deposits of arid climates would give a fifth of the land surface as mantled by continental formations." The lands, in the course of the geological ages, are, however, warped and elevated into mountain ranges, so that the geological record "should show a far less proportion of thin and superficial land deposits." On the other hand, basin and delta deposits should be quantitatively as great as those laid down upon the floor of the epeiric seas.

Having developed the principles of sedimentation for continental and shallow-water marine deposits, Barrell applied them in 1907 to a late Mississippian formation in the paper entitled "Origin and Significance of the Mauch Chunk Shale," in which he concludes that "In the anthracite region, more surely in the southeastern and eastern portions, the whole formation [which is about 3000 feet thick], from top to bottom, was a subaerial delta deposit laid down under a semiarid climate." The nearest approach to-day to a similar area is the highly arid Punjab region near the base of the Himalayas and the lower plains of the Indus River. "These comparisons, while not intended to convey the idea that the Appalachians were ever of Himalayan magnitude, are suggestive of a more massive range of mountains and a wider land area to the eastward of the Pennsylvanian geosyncline than is customarily thought of as existing in Upper Devonian and Carboniferous times."

Having seen much of the Carboniferous of eastern Pennsylvania, Barrell had asked himself, "To what extent have the tectonic movements and climatic variations caused the great contrasts seen here in the Lower and Upper Carboniferous formations?" To solve this problem, he took up in detail the principles that have to do with the relations between climate and terrestrial deposits and published his results in a paper bearing that title. He writes: "The environment of the lands may be classified into three fundamental and independent factors—the relations to the surrounding seas, the topography which forms their surfaces, and the climates which envelope them; each of major importance in controlling the character

of the lands." Fundamental are the relations of the continental fluviatile deposits to the climates, and they may be successfully used in determining those of the geologic past. "This is exclusive of the significance of salt and gypsum deposits on the one hand or of glacial deposits on the other, which are of course universally recognized, but these are the marks of climatic extremes."

The first part of the paper under review has to do with the relations of sediments to regions of erosion. It deals with the relation of physiography to erosion and the consequent supply of waste as sediments to the formations. Then he takes up the relations of sediments to regions of deposition, and finally the relations of climate to fluviatile transportation. These parts lead to the conclusion that "Climate is a factor comparable to disturbances of the crust or movements of the shore-line in determining the nature and the variations in the stratified rocks of continental or offshore origin, thus playing a part of large, though but little appreciated, importance in the making of the stratigraphic record."

Along with many other things, Barrell finds that "While the varying powers of erosion and transportation are delicate stratigraphic indicators of *climatic fluctuations*, the chemical and organic control accompanying the deposition are the more secure indicators of the *average climatic conditions*."

Finally, what was the origin, environment, and significance of the conglomerate and sandstone formations intercalated between others of different nature? Barrell's answer is that these coarse materials have three origins: first, marine conglomerates and sandstones; second, tectonic conglomerates and sandstones; third, climatic conglomerates and sandstones.

"Changes in volume of ocean waters, earth movements, and atmospheric activities are the three mixed and fundamental causes by which the three classes of deposits become possible, but the records which they embody are largely distinct and independent. By separating conglomerates and sandstones into these three classes, the sedimentary rocks, therefore, present a threefold record, the marine conglomerates giving that of the variable relations of land and sea; the tectonic conglomerates,

the record of variable vertical uplifts; the climatic conglomerates, the record of variable temperature and rainfall."

In 1910, Professor Sherzer published a valuable paper on the "Criteria for the Recognition of Various Types of Sand Grains." He found seven types, and in the application of the ascertained principles to the Sylvania sandstone, he came to the conclusion that it is of eolian origin. In a review of the paper in the same year, Barrell thinks this conclusion is not warranted, because as yet too few sands, modern and ancient, have been studied enough in detail.

Barrell's next study was on "Some Distinctions between Marine and Terrestrial Conglomerates," the gist of which he presented before the Geological Society of America in 1908. A half-page abstract appeared in the Society's *Bulletin* and in *Science* in 1910, but the paper as a whole was not printed until 1925. Its conclusions are as follows:

The truly terrestrial forces produce vastly more gravel, pile it in thicker formations, spread it far more widely, and provide more opportunities for accumulation than do the forces of the marine littoral areas. Conglomerates are, therefore, dominantly of terrestrial origin; they are as characteristic of continental deposits as the limestones are dominantly marine.

The present volume rates of denudation Barrell finds to be, for marine areas, between .02 and .10 cubic miles per year; while fluviatile denudation yields as marine or as continental deposits 1.50 to 3.00 cubic miles per year. In other words, the total annual erosion is from about 3.00 to 6.10 cubic miles.

Marine denudation attains its "highest absolute and relative value at times of great marine transgressions. . . . Its absolute value may readily have been twice the present amount, and its value relative to subaerial denudation may have attained even twenty times the present ratio."

In regard to gravels, he says: "The gravels supplied by fluviatile erosion are some tens of times greater in quantity than the gravels produced by the erosive action of the sea."

Concerning the distribution of marine gravels, Barrell's conclusions are: "On open coasts with fixed shorelines marine gravels commonly range from about 1 fathom above high-water level to 4 or 5 fathoms below. They are kept against the coast

and seldom extend more than from 1 to 3 miles from shore. . . . Marine gravels . . . may under exceptional circumstances, extend to ten times these limits."

Marine conglomerates rarely attain a thickness of 100 feet, and even this depth is exceptional and local. On the other hand, terrestrial conglomerates are often many hundreds and sometimes thousands of feet thick.

"Rivers commonly move gravels down descents of hundreds of feet from their regions of origin and along horizontal distances of tens of miles. . . . Under favorable conditions, moreover, rivers may sweep gravels from far inland mountains to the margin of a continent and down descents of thousands of feet. Consequently, rivers, as agents of gravel distribution, may be a hundred times more effective than the shore activities."

"In conclusion, it is seen that gravels of either marine or terrestrial origin require stable conditions for their development. They are local and not widely distributed formations. Marine gravels are most restricted, since they are limited to the margin of the sea. River gravels, on the other hand, may occur over all parts of the lands, but do not tend to attain the sea. Finally, in order that gravel may remain as conglomerate formations, either of terrestrial or marine origin, it must be progressively buried below the zone of erosion."

Having finished the paper on conglomerates, Barrell now sought out a thick and unfossiliferous conglomerate-sandstone series whose age relations were obscure. Such a series he found in the southern Appalachians, and upon this he made another study that was published during 1925. The conglomerates, sandstones, and slates of the Ocoee and Chilhowee groups of Tennessee, North Carolina, and Alabama have long been a stumbling block in correlation, because it is only near the top of the thick series that fossils have been found. On the basis of these fossils, Keith finally referred all of the Ocoee and Chilhowee to the Lower Cambrian. Barrell discussed the many formations of this series, aggregating between 9000 and 13,000 feet in depth, and concluded that the lower part, with a maximum thickness of 7500 feet, is of terrestrial origin. The middle formations, with a thickness of 3400 feet, are in

part at least a terrestrial deposit, though a part is probably estuarine. The remaining 2700 feet are entirely of marine origin.

As to the nature of the geosyncline in which these Ocoee-Chilhowee deposits were laid down, he concludes that it lay between mountains, had a very irregular bottom, and that its crustal subsidence shifted at first from place to place. Most of the deposits came from the areas of maximum deposition, tens of miles away. In regard to the geanticline that supplied the Ocoee-Chilhowee formations, he thinks that at the close of the Proterozoic an Alpine-like mountain system stood on what is now the Piedmont Plateau of the southern Appalachians. Finally, late in Lower Cambrian time, the region passed into topographic old age. The climate, he holds, was at first cool, with moderate rainfall, and later considerably warmer.

In 1912, in continuation of his studies of sedimentary formations, Barrell published the paper entitled "Criteria for the Recognition of Ancient Delta Deposits." He defines a delta as "a deposit partly subaerial built by a river into or against a body of permanent water." This study concerns the detailed structures of deltas, the physiography of the lands that furnished the detritus, and the cyclic nature of the erosion of the rivers and therefore also of the delta accumulations. It is a difficult study because of the great variability in the extent of deltas, in the character of their sediments, in the size and streaming power of the river or rivers that bring the material, and finally in the wave and streaming forces of the water body and the depth of the water in which and in front of which the deltas are laid down.

In 1913-1914 followed the application of the criteria of the previous paper to the Upper Devonian delta of the Appalachian geosyncline. This is one of Barrell's best pieces of work, and a very philosophic one, for it brings out the relations of the Appalachian delta, both to the interior sea and to the extensive eastern land Appalachia and the Atlantic Ocean beyond. The volume of this extensive delta Barrell computed at about 16,500 cubic miles for Middle Devonian and 63,000 cubic miles for Upper Devonian time. "This is an impressive measure of the volume of the adjacent land which was eroded in Upper

Devonian times. But it is a minimum measure, since that part of the rocks which was taken into solution was carried farther away, and of the mechanical sediments it represents only that part which was carried westward into the trap of the geosyncline."

This Devonian delta implies a much greater Appalachia than is usually assumed, and one which must have exceeded the present Sierra Nevada in elevation. Barrell thinks that this old land was not confined to the limits of the present continental shelf, but probably extended out into the Atlantic for an unknown distance where there is now deep ocean. Accordingly, great parts of eastern Appalachia must have been fragmented and sunk into the depths of the Atlantic during Mesozoic time.

As early as 1905, Barrell began to ask himself, What were the geographic and climatic conditions which controlled the nature of the Old Red Sandstone deposits? In 1906 he wrote out his views but withheld them from printing, thinking that he and the present writer would find means of visiting Scotland. As this opportunity did not come, he presented his ideas in 1916 in the paper "Dominantly Fluviatile Origin under Seasonal Rainfall of the Old Red Sandstone." A copy of this paper was sent to Professor T. G. Bonney of University College, Cambridge, England, and his letter of thanks to Barrell opened with the word "Eureka!" Truly, here is the correct explanation of the origin of the Old Red Sandstone.

"The central conclusion reached in this paper is that the Old Red Sandstone formations were not deposited in lakes or estuaries, nor are they of desert origin." They are "river deposits accumulated in intermontane basins," "exposed to air in times of drought," and "similar to the basin deposits of the western United States laid down in the Tertiary period between the growing ranges of the Cordillera." There was "an alternation of seasons of rainfall and drought—a climate with an arid season, but not an arid climate." "The Great Valley of California may therefore in the present epoch, both in physiography and in climate, be cited as a striking illustration of the nature of the Old Red Sandstone basins."

In 1915, R. W. Sayles presented a paper on "Banded Glacial Slates of Permocarboniferous Age, showing Possible Seasonal Variations in Deposition." In discussing this paper, Barrell pointed out that he had noticed similar stratigraphic banding in argillites associated with the oldest of all known tillites, those of the Huronian of Ontario. He says: "At the south end of Cobalt Lake occurs a thick bed of argillite delicately banded, indicating rhythmic deposition. The bands are grouped in series which show larger rhythms. If the bands are annual, the rhythmically recurring groups show climatic fluctuations covering periods of years. . . . The association of these banded argillites with iceberg deposits and ground moraine indicates, therefore, the existence of summer melting and winter freezing during the Huronian glaciation."

In 1908, Professor Schaeberle published his conclusion that most of the postulated theories of geological climates are upset by his theory of temperature of space, that is, they are invalid because they are based upon an adopted value for the temperature of space which is too great by nearly three hundred degrees of the centigrade scale at the earth's distance from the sun." To this sweeping dictum Barrell, in a short notice in *Science,* "Schaeberle and Geological Climates," replied that ancient climates "are based not upon considerations of the temperature of space, but upon detailed studies of the geological record." "Manson's hypothesis of an earth self-heated and protected by a cloud envelope until the Tertiary, which Schaeberle considers that he has demonstrated as a true theory, must be regarded as in no measure adequate to explain the facts."

Paleoclimatology as derived from the nature of the sediments was Barrell's special field, and he was the recognized leader in it. His article on the relation between climate and terrestrial deposits (1908), discussed more fully on page 16, "traces," according to Willis, "the complex variations of temperature and rainfall and their effects upon sediments with such keen analytic power, such wealth of illustration, and such logic as to lay firmly the foundations of interpretation of terrestrial stratigraphy in terms of climate."

PIEDMONT TERRACES OF THE NORTHERN APPALACHIANS

While an undergraduate student at Lehigh, Barrell became interested in the physiography of the highlands of New Jersey and Pennsylvania. He had studied Davis' works, but on account of the peculiarities of the wind gaps and the rivers that flow through the ridges with which he was familiar, he concluded that much of the area must have been beneath the sea and covered by sedimentary deposits. This was in opposition to the prevalent opinion that the present rivers were incised in the "Cretaceous peneplain." His views were formulated for the first time in his Lehigh thesis of 1897, but it was not until 1913 that he presented the matter in more mature form before the Geological Society of America. The assumed Mesozoic terrestrial peneplain of southern New England he finally showed to be in reality of marine origin and "stairlike or terraced in its character, facing the sea." It was this study that was absorbing his last years, and his death made impossible what would have been his *magnum opus*. In 1915 he demonstrated his theory in the field before the New England Intercollegiate Geological Association, as leader of a characteristically well planned and carried out excursion to the critical areas in Connecticut, in the course of which he met with equanimity the frequent questions and objections of a party which included Davis, Johnson, Miss Bascom, and many others with vigorous ideas of their own on the subject.

Regarding this intricate and far-reaching study, Barrell left a mass of manuscript notes and drawings, which H. H. Robinson put into order for publication during 1920. Barrell had not, of course, gone far enough with his studies to develop his ideas completely, but he laid the foundations for others to build upon. The subject is a fascinating one, and all the more so since Barrell's views are decidedly at variance with the generally accepted views of geologists, both as to the number of erosion cycles and their ages. The following is taken from Robinson's editorial review of Barrell's work:

"The physiographic history of the Appalachian region has rested on the recognition of a topographic plane of reference, commonly known as the Cretaceous peneplane, above which

rose older residual masses, below which were locally cut secondary topographic levels of Tertiary age representing temporary stillstands of the land following periods of uplift, and on the outer margin of which rested the great unlithified series of Mesozoic and Neozoic sediments. The underlying assumption was that all peneplaned surfaces were the result of fluvial denudation and that remnants of all remain so little reduced as to permit the former surfaces to be restored with assurance.

"In most parts of the province three cycles of peneplanation were recognized and correlated in a general way, as the Cretaceous, the early Tertiary, and the late Tertiary. In some localities an older erosion surface, the Jurassic peneplain, was recognized."

Barrell held, at least for New England, that the various terraces are "initially the result of marine denudation," that most of them are of post-Miocene age, and "that the terraces could be restored from their existing remnants." Gradually he came to the conclusion that there were eleven such benches, all of marine origin. "The names, elevation [in feet] of the inner restored margins, and the age of the terraces are as follows:

"Becket	2450	Cretaceous
Canaan	2000	Cretaceous
Cornwall	1720	Oligocene
Goshen	1380	Pliocene
Litchfield	1140	Pliocene
Prospect	940	Pliocene
Towantic	740	Pliocene
Appomattox	540	Pliocene
New Canaan	400	Pleistocene
Sunderland	240	Pleistocene
Wicomico	120	Pleistocene"

Robinson concludes: "A careful study of the profile and maps both topographic and geologic, coupled with some acquaintance with the region, has left the definite impression that Professor Barrell's conclusion as to the marine origin of the terraces below the Cornwall is well taken." The two older terraces, however, Robinson regards as of early Tertiary origin rather than as of Cretaceous time.

RHYTHMS AND GEOLOGIC TIME

Barrell was one of the participants in a symposium on the interpretation of sedimentary rocks at the Albany meeting of the Geological Society of America in 1916. Here he presented a part, but only the smallest part, of a study on "Rhythms and the Measurements of Geologic Time." This is his most important work, and will remain a source of information and stimulation to research along several lines of philosophical thought.

He had long been attracted by the cycles of sedimentation: "Nature," he says, "vibrates with rhythms, climatic and diastrophic." The viewpoint of the six parts of the study is geological, with the emphasis on the pulsatory nature of uplifts and subsidences, though the evidence furnished by radioactivity is thoroughly reviewed. Part I treats of the rhythms in denudation, and shows that "erosion is essentially a pulsatory process" and that "a single rhythm is the erosion cycle; and small partial cycles are superimposed on larger." Here then is developed the hypothesis of compound rhythms. This part leads to the conclusion that the present rate of denudation is high, in fact, "very much greater than the mean for geologic time." Part II deals with rhythms in sedimentation, and shows that sedimentation "is not a continuous process, even during a stage of crustal depression." Therefore the stratigraphic record is replete with "breaks" of varying time lengths, the non-seeable greater disconformities and the lesser but more numerous diastems.

J. M. Clarke in "L'Île Percé" says: "My lamented friend, Joseph Barrell of Yale, wrung the music from a wall of rock of water-laid sediments, by showing how the record, if correctly read, is a succession of rests and beats. The quiet waters lay down their deposits in uninterrupted conformity, till a storm wave of short length and greater height breaks them. The high note passes and subsides into the lower beat, again recorded in unbroken deposits. Slowly the sea bottom is raised above the waves and the hardened sands and clays are worn jagged and rough by the rains and streams; slowly the land sinks beneath the waters and on its uneven strata are again

laid the even deposits of the quiet sea. Thus about him everywhere, if one will read and listen, lie the symphonies of the rocks."

Part III treats of the estimates of time based on geologic processes, on erosion, sedimentation, hypothesis of compound rhythms, amount of oceanic salt, and on the loss of primal heat. In this presentation we get a more adequate idea of the quantitative lengthening of geologic time. "Measurements of Time based on Radioactivity" is the subject of Part IV, which is a worthy associate of Holmes's "Radioactivity and the Measurement of Geological Time," published in 1915. Finally, in Part VI, "Convergence of Èvidence on Geologic Time and its Bearings," the geological and physical arguments are bound together into a unity, resulting in the conclusion that at least 550 million, and a maximum of 700 million, years have elapsed since the beginning of the Cambrian. This is, moreover, less than one half of geologic time, for the Laurentian or post-Ladogian granites, the oldest great invasions of igneous magmas into vast thicknesses of sedimentary formations, have an age as great as 1,400,000,000 years. "Combining the indications regarding the present high rate of denudation with the evidence of the halting and discontinuous nature of past deposition, it is seen that geologic time is certainly much longer—perhaps ten or fifteen times longer—than the estimates based on strictly uniformitarian interpretation." This paper, Holmes (1925) says, is "the most careful study of geologic time yet published."

Harlow Shapley, the astronomer, accepts Barrell's geologic time estimates, and says: "We may study the stars, indeed, with the aid of fossils in terrestrial rocks, and acquire knowledge of atomic structure from the climates of Precambrian times." "In the growth of our concepts of the age of the earth, Barrell's discussion is likely to mark an epoch because of its consistent carefulness, its great expansion of geologic time beyond the commonly accepted limits, and its decided rebellion against the stringent limitations set by Kelvin and later physicists."

ISOSTASY

Barrell's most philosophic works have to do with isostasy and the genesis of the earth. It was in 1904 that he began to write on the former subject, in a review of T. M. Reade's book, "The Evolution of Earth Structure." Two years later he wrote a review of J. F. Hayford's "The Geodetic Evidence of Isostasy," giving also a brief historical review of the place of this subject in geological literature and of the problems upon which it bears. Finally, in 1914, he reviewed briefly the status of the hypothesis of polar wandering, concluding that "Closer examination tends to cut down more and more even those moderate limits of polar migration set by Darwin. It would appear that the assumption of polar wandering as a cause of climatic change and organic migrations is as gratuitous as an assumption of a changing earth orbit in defiance of the laws of celestial mechanics."

During the years 1914 and 1915, Barrell published in the *Journal of Geology* a series of eight papers that were later collected and bound in one volume under the title "The Strength of the Earth's Crust." The first part of this series of articles treats of the geologic tests of the limits of strength. In Parts II and III is discussed the "Regional Distribution and Influence of Variable Rate of Isostatic Compensation." Part IV deals with the "Heterogeneity and Rigidity of the Crust as Measured by Departures from Isostasy," while Part V is on "The Depth of Masses producing Gravity Anomalies and Deflection Residuals." Part VI is devoted to the "Relations of Isostatic Movements to a Sphere of Weakness—the Asthenosphere," and Part VII to "Variation of Strength with Depth, as shown by the Nature of Departures from Isostasy." The final part has to do with the "Physical Conditions Controlling the Nature of Lithosphere and Asthenosphere."

This work at once placed Barrell high among geodesists and geologists. Willis says: "Barrell's analysis covers every part of the subject exhaustively, dissociates all its elements, weighs them, and recombines them. It is the product of extraordinary industry, activity and thoroughness." Arthur Holmes writes of it as a "remarkable series of papers, which is worthy of the

most careful study, and constitutes a valuable and stimulating contribution to terrestrial dynamics." According to Pirsson, "They constitute probably the most serious and profound discussion, which has yet been attempted, of the facts which are known and of the theories which have been deduced from them, concerning the strength of the earth's outer shell."

The year of Barrell's death, there simultaneously appeared two papers, "The Nature and Bearings of Isostasy," which is a non-technical summary of the subject, and "The Status of the Theory of Isostasy." These papers, which may be regarded as Barrell's mature views on isostasy, are reviewed in *Nature* for February 12, 1920, and from that review the following excerpt is quoted: Barrell here maintains "that surface inequalities of contour and mass are accompanied by inverse inequalities of density beneath the surface, so that at a depth of about 120 kilometers equal areas have equal masses superposed." He "contends that over limited areas there are large deviations—amounting to 1000 feet over an area 200 miles in diameter (about 3°), or even more. He regards the upper part of the earth's crust as sufficiently strong to sustain uncompensated loads of this amount, the vertical magnitude of the departure being, of course, inversely proportional to its areal extent; it can thus support individual mountains or limited ranges, as well as erosion features of considerable magnitude, such as the Nile and Niger deltas. Under greater and more widely extended loads, however, the crust is supposed to bend in gentle curves involving but little crustal stress; this bending is accompanied by yielding in a lower, weaker layer, which brings about isostatic compensation."

Barrell's work on isostasy, Bowie states, "was far-reaching and thorough, and threw much light on the relation of isostasy to geology. In fact, Barrell did more, in my estimation, to bring the geodesists and geologists together in the science of isostasy than did any other investigator."

GENESIS OF THE EARTH

In 1907, Barrell reviewed W. H. Pickering's paper, "The Place of Origin of the Moon." This place the latter thought to

be the Pacific Ocean, thus giving rise to that basin. Barrell shows that oceanic basins could not have arisen in this way.

Chamberlin's very important book, "The Origin of the Earth," appeared in 1916, and Barrell was looking for it. After reading it, he wrote a five-page review which was printed in *Science* for August 18, 1916. "The subject is vast," he says, "and the evidence on many aspects is somewhat vague. A variety of subhypotheses could be raised for comparison." Regarding the size of the planetesimals, Barrell states: "It seems a debatable question to the reviewer if such a large proportion of the added material was necessarily dust-like and capable of being distributed by the primitive atmosphere and ocean. Upon the mean size of the incorporated units various subhypotheses of consequences may be built up."

Chamberlin's hypothesis of juvenile shaping "is, of course, like the other steps in the development of the planetesimal hypothesis, dependent upon the basal postulates. It is not clear that earth-strains due to the causes invoked could initiate such a primary segmentation . . . in fact, calculations on the stresses which the reviewer has made to test this subhypothesis pointed to quite a different method of yielding. The distribution of continents and oceans does not accord very closely with it, and the evidence of isostasy does not indicate that the density differences between continents and ocean basins reach below the outer fiftieth of the earth's radius."

In Barrell's opinion, however, "The Origin of the Earth" is "a notable constructive addition to thought upon this fundamental subject. . . . The names of Chamberlin and Moulton must rank high among those scientists who have dealt constructively with that vast, vague and remote problem—the Origin of the Earth."

When Chamberlin's book appeared, Barrell was getting ready for a lecture to be delivered on November 23, 1916, before the Yale Chapter of the Sigma Xi, the introductory one in the course on "The Evolution of the Earth and its Inhabitants" (later issued in book form by the Yale University Press). The manuscript finally grew far beyond the content of his lecture, and eventually appeared in part in three different places: as the first chapter in the book resulting from the lectures; as Chapters

VIII, IX, and XI in the first edition of Schuchert's "Historical Geology"; and in the *American Journal of Science* during 1927.

In the summer of 1916, Barrell wrote on the "Significance of the Equatorial Acceleration in the Sun's Rotation." This short paper will not be published, but is preserved for future reference in the library of Peabody Museum, Yale University. It was followed by an extended paper entitled "Geological Relations of Earth Condensation and Resulting Acceleration in Rotation," which was published in 1925 and 1926. He begins with the questions: "(1) Is the earth smaller now than it was in the early geological periods? If so, what has brought about that condensation? (2) Has tidal retardation by moon and sun effectively increased the rotation period of the earth since the Archeozoic, or has this been a geologically negligible factor? (3) Has condensation produced an acceleration in rotation? If so, how much more oblate would the earth become? (4) Could such a change of figure be recognized by a change of sea-level and to what amount, or would it be inappreciable in effect? (5) What distortions would be given to the crust and would these be wholly masked by the orogenic and epeirogenic movements due to other causes? (6) How would the stresses due to change in figure be distributed through the earth? Where and how would failure occur? (7) Are regional joint-systems related to the planetary stresses? (8) Lastly, can these forces have reached a higher order of magnitude during the formative period of the earth; and if so, may they have had an influence in determining the arrangement of the larger features of the earth?"

His conclusions are divided into two groups. On the one hand, so far as stresses in the earth's body are concerned, those due to change of oblateness would appear to be wholly submerged beneath those due to change of volume. It is difficult to see in these results either a basis for a segmentation of the earth into great conical sectors, or a recognizable cause for the shifting of epeiric seas. On the other hand, it appears probable that planetary strains occur in the outermost shell of the crust, the zone of fracture, which may constitute one of the major factors in determining the nature of regional joint sys-

tems. These stresses may also coöperate with local forces in helping to determine the trend of normal fault systems.

"The paper is written chiefly on account of its suggestiveness toward further work, rather than because of any finality in conclusions. In a field where so little is directly known, as in that of the deep interior of the earth, the method of multiple working hypothesis must be assiduously cultivated, and although the present analysis favors one group of hypotheses, it should not be regarded as ruling others out of further consideration."

Finally, Barrell wrote "On Continental Fragmentation and the Geologic Bearing of the Moon's Surficial Features." This paper, published in 1927, includes his ideas of how land bridges like western Gondwana across the Atlantic may be dragged into the depths of the oceans. "The outer crust of the earth," he says, "granitic in its upper part and somewhat more basic at depth, is held to have a thickness of from 50 to 75 miles. It is very strong, and is marked by broad variations in density amounting to as much as 5 per cent, and by more local variations up to 10 per cent, these differences corresponding to the broader relief of the earth's surface. Below this lies a thick, hot, basic, rigid yet weak shell, the asthenosphere, or sphere of weakness. The problem of the origin of the ocean basins and the continental platforms consequently resolves itself into one of the origin of the density differences in the lithosphere and the maintenance of the heated and weak condition in the asthenosphere."

"The small content of radioactive elements in the basaltic shell or asthenosphere below the granitic crust of the continents would then supply that slow increment of heat which is necessary to generate new molten rocks. The granitic shell loses its excess heat by conduction to the atmosphere, but the asthenosphere is so deeply buried that its heat can not escape, but must slowly transform some of the solid rock into liquid form. Reservoirs of molten rock gather until their mass, combined with their decreased density in the fluid form, enables them to work their way into and through the lithosphere and demonstrate their existence in igneous activity at the surface of the earth. The magma which thus comes from the greatest depth and in greatest volume would, because of the initial density

stratification, produce a notable increase in the density of the outer crust. In order to reëstablish isostatic equilibrium, such a region must subside."

"Most American geologists hold strictly to Dana's theory of the permanency of the continents and ocean basins, whereas European workers in general stand by the older view that ocean basins are broken-down portions of the granitic shell. We may also include this grander process of crustal change under the term of continental fragmentation. Great intercontinental troughs, such as the Red Sea and the Caspian, are thought to have been made in later geologic times by fracture of their margins and subsidence of their floors. The writer accepts the European view, since, in spite of its difficulties, it yet accounts for many geological relationships. If continental fragmentation is real, it has a strong bearing upon the general problem of the origin of ocean basins, for the progress of fragmentation is in reality a continuation of the formative process. Through fragmentation the margins of the continents break down into the oceanic depths and enlarge them, and at the same time diminish the areas of the land."

Finally: "Smoothing out the crustal oscillations connected with periods and eras, it appears that a great cycle of progress has run through earth history. High and wide lands marked the Archeozoic and Proterozoic revolutions; fragmentation was apparently widening the earliest ocean basins and lowering the ocean levels, but the juvenile waters from the accompanying igneous intrusions reëlevated them. Then came a long time, from the early Paleozoic to the close of the Devonian, during which the oceans rose and repeatedly spread over the lands. Since the later Paleozoic, however, the ocean level has tended to sink, and fragmentation appears to have gained on the accessions of juvenile water. . . . But the lands have been kept high above the increasing waters only by the breaking down of portions of their areas into ocean basins. . . ."

Barrell then takes up a study of the moon's surface as known to astronomers and ends the paper of 1927 as follows: "Thus the study of the earth's small sister planet supports the general hypothesis that the ocean waters as well as the ocean basins have arisen through igneous activity, and that fragmentation of

the original crust has dominated the moon as well as the earth throughout geological time."

In the Sigma Xi lecture mentioned above, Barrell modified the Chamberlin hypothesis of slow growth resulting in a cold earth to one of quick growth through planetoids the size of asteroids, a postulate which necessitates a molten earth.

PALEONTOLOGY AND EVOLUTION

As Barrell also taught Biology at Lehigh and Historical Geology at Yale, it was but natural that he should be interested in Paleontology. This side of his activity is little known away from Yale, but his colleagues there were often made aware of his deep interest and knowledge along this line. He never was concerned with species and genera, nor with classification, but to him the bony structures of vertebrates were mechanisms and he tried to see in them the effects of the operation of the laws of mechanics. And through his insight into Paleoclimatology, he tried to discern the effects of changing environment as the most important cause of organic evolution.

While studying the nature of the Old Red Sandstones, and what they show as to their climatic environment, Barrell became deeply interested in Chamberlin's ideas regarding the probable habitats that gave rise to the fish-form, and to lungs. His ideas on these subjects culminated in 1916 in a paper entitled "Influence of Silurian-Devonian Climates on the Rise of Air-breathing Vertebrates." The problems he seeks to answer are two: "first, as to the environment in which fishes developed; second, the changes in the environment and the associated organic responses which brought forth amphibians from fishes. It is the solution of the second problem which is here especially sought."

"It is shown to be probable that fishes arose in land waters. As such they constituted primarily a river fauna." It is in the Middle Devonian that the fishes "first really begin to conquer the ocean and its former rulers." On the other hand, in the fresh waters of this time fishes abounded in greater variety than in the seas. In the Upper Devonian, crossopterygian fishes had risen to a dominant place, and they were adapted to

living in warm climates marked by alternation of wet and dry seasons, the kind of environment that gave rise to the amphibians. "The warm and stagnant waters of the dry season compelled those fishes which should survive to make larger and larger use of air."

"The evidence is regarded as strong that the air-bladder was originally developed as a supplemental breathing organ, although in modern fishes it has been mostly diverted to other uses." Barrell also quotes this significant passage from W. D. Matthew: "The evolution of land life in adaptation to recurrent periods of aridity supplies a satisfactory background of cause for the whole evolution of the higher vertebrates," to which he adds, "Climatic oscillation is a major ulterior factor in evolution."

The study of the natural environment as recorded in the sediments that also entombed the fossils led naturally to the work entitled "Probable Relations of Climatic Change to the Origin of the Tertiary Ape-Man." Here we again read that climatic variation is the most fundamental evolutionary factor for terrestrial life. This was especially true for the Pleistocene, when the land biotas "have come and gone at the command of climatic change. Those animals which were trapped on the northern sides of mountain ranges or water barriers were remorselessly exterminated by the waves of advancing cold; those which could escape to the south returned with milder climates, but changed in assemblage." Barrell held that man was brought to his present high physical and mental state not as the "mere product of time and life," but that he is "peculiarly a child of the earth and is born of her vicissitudes." "The progress of life on the earth has been highly favored, consequently, by the rhythmic pulses of diastrophic and climatic changes which have remorselessly urged forward the troop of living creatures. The progress of organic evolution has depended upon a series of fortunate physical events, conditioned in the internal nature of sun and earth, rather than the byproduct of mere life activities as expressed in orthogenesis through long periods of time. Evolution is in no sense an inevitable consequence of life, and the compulsion of climatic change has been

more than once a fundamental factor in the age-long ascent from protozoan to man."

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(In files of the U. S. Geological Survey.)

Description of the Housatonic quadrangle.

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
VOLUME XII — SECOND MEMOIR

BIOGRAPHICAL MEMOIR

OF

WILLIAM FRANCIS HILLEBRAND
1853-1925

BY

FRANK WIGGLESWORTH CLARKE

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1925

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BY FRANK WIGGLESWORTH CLARKE

The following memoir falls naturally into three parts. First, an outline of his life, prepared by Hillebrand for the use of the National Academy of Sciences. Second, a discussion of Hillebrand's career as a chemist, by the present writer. This includes a few lines of preface, covering some omissions in the autobiography. Third, a bibliography prepared by Hillebrand himself.

AUTOBIOGRAPHICAL NOTES

BY W. F. HILLEBRAND

I was born at Honolulu, December 12, 1853. My father, William Hillebrand, was born at Nieheim, near Paderborn, Westphalia, Germany. His father held a judicial position. Educated for the medical profession at Göttingen, Heidelberg, and Berlin, my father, one of four brothers, and the oldest, I think, on account of his health, took a sea voyage to Australia late in the 40's, and thence, at a date unknown to me, went to Manila, where he practiced his profession for a time, but becoming very ill, had to seek another climate. He embarked for San Francisco on a brig with little expectation of living to reach his destination. He did, however, and was advised to seek the Sandwich Islands, where he must have arrived about 1851. By the exercise of great care he recovered his health completely. In Honolulu he met and married Anna Post, step-daughter of Wesley Newcomb, a well-known conchologist. From this union sprang two sons, myself and Henry Thomas, the latter over 14 years younger than myself.

In the practice of his profession my father became perhaps the most prominent physician in the Islands, and filled several positions of importance, besides being one of the Privy Council of King Kamehameha the Fifth. An ardent botanist and hor-

ticulturist, he explored the Islands thoroughly and introduced from various parts of the world many plants and trees new to the kingdom. His garden became the show place in Honolulu from the horticulturist's point of view. In 1865 he was sent, my mother and I accompanying him, to investigate labor conditions in China, Java, and India, with a view to securing a supply of efficient labor for the sugar plantations of the Islands. While on this trip he dispatched two shiploads of Chinese coolies from Hongkong, which was the beginning, on a considerable scale, of what became a strong influx. While on this trip he made large collections of living plants and birds, most of which reached their destination in fair shape and not a few became thoroughly at home in their new habitat. I have very vivid recollections of helping to feed and care for the birds and deer on the seven weeks' trip from Hongkong to San Francisco by sailing vessel, a somewhat rough and gloomy voyage.

In the late 70's, while living in Madeira, he inaugurated the emigration of Portuguese laborers with their families from that island to Honolulu—a movement which, rendered difficult at the start by the opposition of the Portuguese Government, took on considerable proportions when the first emigrants sent back favorable reports.

In 1870 my mother, brother, and I left the Islands, I to enter Cornell University preparatory to completing my education in Germany. A year later my father followed us and spent a year in studying botanical collections, chiefly with Prof. Asa Gray at Harvard, as a preliminary to entering upon the preparation of a Flora of the Hawaiian Islands. In the summer of 1872 we all took ship for Europe by way of Scotland; thence to Hamburg, Bremen, Braunschweig, Cologne, and Bonn, and eventually Heidelberg, where I entered the University in November, 1872. The family remained in Heidelberg for a few years, but my mother's health became bad and all but myself sought the milder climate of Madeira, and later Teneriffe, whence, as my mother's health became better, they returned to Europe and lived in Switzerland, and then in Heidelberg for a time. During most of this time my father was engaged in

writing the Flora. It was at Heidelberg that he died, on July 13, 1886, just after he had placed the text of the work in the hands of the printer. This work it was my pleasure and duty to edit and see through the press, with the assistance of my father's friend, Prof. E. Askenasy, of Heidelberg. Some of the foregoing information and some additional data are to be found in the editor's preface to the Flora.

My early education was acquired at Oahu (Punahou) College, near Honolulu, under the chief guidance of Prof. W. D. Alexander, with exception of the two years, 1862 and 1863, at the College School in Oakland, California, at the head of Webster Street on 12th. My grandmother and step-grandfather Newcomb were then living in Oakland, and I had already visited them in 1861 without my parents. It may here be remarked that my grandmother was descended from the Wells family, of Colchester, Conn. My maternal grandfather, William Stoughtenburgh Post, whom I never saw, was a descendant of a Quaker family of Long Island by way of Hyde Park, Dutchess County, New York.

Latin, Greek, French, elementary mathematics, composition, and spelling comprised essentially my school curriculum, which studies were continued at Cornell, with German added. My father was a fine musician as well as botanist, but I did not inherit has tastes, although I loved to accompany him on his excursions through the dense forests and up the lofty mountains of our island home, and was zealous in aiding his search for new and rare plants and flowers. Not seldom did I make such botanical all-day excursions by myself or with a boy companion, sometimes at considerable risk. Botany as a subject of study interested me not at all, and for music I had but an indifferent ear and taste, though fond of it if not ultra classical and to me devoid of melody.

Up to the time of reaching Bonn, in the summer of 1872, I had shown no bent or aptitude, unless a liking for geography, travel, and history can be so called, for any line of study that pointed toward a choice of a profession or career. Mathematics I abhorred, and English composition was something that there seemed little hope of my ever becoming master of.

My inability to get a good working understanding of mathematics has been a serious handicap throughout my professional career. My actual study of English was limited to the merest elements, my father laying much greater stress upon the classics. While the study of the latter has been of much assistance and of no little cultural benefit, I can not too much deplore the scant consideration that was given to English. My father, though himself a master of English, I think was inclined to believe that my later home would be in Germany, notwithstanding that all my youthful associations were with Americans and my predilections for America. This bias arose from the great preponderance of Americans over Europeans in Honolulu, from the fact that all my early teachers were Americans, that the language of the family was English, and that I had frequent association and correspondence with my mother's family in California and none with my father's, except two of his brothers—one in California, the other in Honolulu, who married American wives. Out of all this grew a strong intention to be and remain an American myself.

On arriving in Germany in the summer of 1872 it became necessary to choose a career. Having seen much of a physician's life through my father and step-grandfather Newcomb, medicine offered no charms. Law was still less to my liking, for the reason that I lacked utterly the mental qualifications of a successful lawyer. The engineering professions were excluded from consideration by reason of my weakness in mathematics. The outlook was unpromising. One day my father suggested chemistry—a possibility that had never occurred to me. In Honolulu I had learned some of the simplest elements of the science and had enjoyed the study. My father's suggestion appealed to me and I decided then and there to become a chemist, or at least to make a try at becoming one. Fortunately for me, in those days a considerable knowledge of mathematics was not considered essential for a chemist. I matriculated at Heidelberg in November, 1872, and studied under Bunsen, Kirchhoff, Kopp, Blum, Leonhard the younger, and Karl Klein, and listened to an occasional course under Treitschke and one or two others. After five semesters I took

my degree (Ph. D.) *summa cum laude,* March 2, 1875, and with the three first named as my examiners continued on what we here would call postgraduate work under Bunsen.

I must not fail here to pay tribute to my friend and close associate at the time, Thomas H. Norton. He it was who prompted me to prepare with him for examination in advance of the time contemplated; and for three months before examination we spent all our spare time together, quizzing each other in German every night until midnight, then for recreation indulging perhaps in a game of chess, at which I was the unfailing loser. Norton was more mature than I and of superior mental power, and it was to his unfailing help that my success at that time was largely due. With him during the next semester I did my first research work, and had justice been done our joint paper "On Metallic Cerium, Lanthanum, and Didymium" would show him as the chief author. My second research, on the specific heats of the metals named, was an outgrowth of the first and was done alone. Both papers were published in Poggendorff's Annalen and the second was copied in translation by the Philosophical Magazine.

After two semesters of this research work I studied for three semesters at Strassburg, chiefly organic chemistry under Fittig and microscopical petrography under Rosenbusch. The organic branch of chemistry did not appeal to me and I made less use of my opportunities than would otherwise have been the case. Nevertheless, with Fittig's help, I determined the constitution of quinic acid and published also a short paper on the crystallography of one of its derivatives, assisted herein by Prof. Paul Groth, who kindly placed the facilities of his laboratory at my service.

Fully intent on returning soon to America and impressed with the opportunities for young chemists as assayers in the western mining regions, I decided to finish my studies by a course in metallurgy and assaying at the mining academy of Freiberg in Saxony. This, as it turned out, was a most wise move, for supplementing the experience in mineral analysis gained with Bunsen it opened the way to an employment which fitted me

in no small degree for my earlier work subsequently with the U. S. Geological Survey.

In the fall of 1878 I returned to the United States and spent the following nine months in Ithaca, N. Y., where my grandparents were living, and in New York City, looking about for a situation. On a visit to Philadelphia I first met Professor Genth and his assistant, Edgar F. Smith. Professor Genth's fame as a mineral analyst was well known to me, and on that account I felt specially drawn to him, our tastes being so similar in respect to mineral analysis. I should perhaps say here that at Heidelberg I had met and seen much of Edward S. Dana. He and I in 1873 took a foot trip through the Eifel and to the Kaiserstuhl, in the Rhine Valley, and thus my attention became first drawn to mineralogy, in which Dana had already had a good deal of experience.

In the spring of 1879 the press had a good deal to say about the new organization of the Geological Survey under Mr. Clarence King. This seemed to be a better opening than a university position or one in the industries, which at that time employed few chemists; so I wrote to Mr. King, transmitting recommendations from Bunsen and President White, of Cornell University. No answer came until months later, when at Leadville Mr. King wrote from Salt Lake City that it had not been determined until then whether or not there would be an opening for a mineral chemist. The decision was now in the negative. An interesting incident of the spring was a trip to Trenton, N. J., at the invitation of Mr. Henry Villard, the widely known railroad reorganizer, whom I had come to know in Heidelberg, where his family sojourned for some time. The trip was made in a special car, with a few other invited guests, for the purpose of witnessing what was perhaps the first test of firing a boiler by a jet of "atomized" oil.

In Lansingburgh, where I spent some time with friends, I met a Mr. Ballou from Leadville, Colo., where he and a brother operated an ore-sampling plant. His advice helped me to reach a decision that I had been tending toward; so late in June 1879, I went to Denver, where I sought advice from Senator Hill, to whom I had an introduction. He himself had nothing

to offer, and I soon went to Central City and Georgetown. At Central I met an old Freiberger, brother of a student I had known in Heidelberg, who was assayer at an outlying mine, and one day accompanied him to the mine and helped him with his day's work, and so learned how different the conditions of assaying were from them as I had learned the art in Freiberg. My friend and everyone talked of little else but Leadville, where a number of Freibergers were then employed in mines and smelters, among them one or two whom I had known at Freiberg. It seemed the place to go to, so I soon found myself there, and within a few days formed a third member in the firm of William S. Ward and Robert E. Booraem, Ward wishing to retire and remain for a time as a silent partner. Both he and Booraem retired in the course of the year and I continued the business alone till July, 1880.

During the year I had met Mr. S. F. Emmons, in charge of the Rocky Mountain Division of the U. S. Geological Survey, who came occasionally to my office for an assay. About the time of the great miners' strike, when business became bad, Mr. Emmons asked me if I would like to take a position as chemist in his division of the Survey. Thus was offered me from a clear sky the very position I had sought when applying to Mr. King. Naturally I accepted the offer and for a month lived with Mr. Emmons and his two geologist assistants, Cross and Jacobs, in a comfortable cottage which had been rented while they were on field service in the Leadville district, the main office being in Denver. During this time I collected samples of ores at the smelters for later examination in the laboratory, which was to be created in Denver. For some time after reaching Denver I was occupied in planning and then supervising for Mr. Richard Pearce the building of a one-story brick structure on the bank of Cherry Creek where Lawrence Street crosses it. The site was on the southeast corner of the block on which the building stood. This laboratory Mr. Pearce rented to the Survey and in it I worked first with Antony Guyard and later with L. G. Eakins. From July 3, 1880, till November, 1885, I remained in Denver; then was transferred to the Washington laboratory, which had been established in 1883, under F. W.

Clarke, Chief Chemist. On July 1, 1909, I entered on duty at the Bureau of Standards in Washington, as Chief Chemist, succeeding after an interval my predecessor, William A. Noyes.

From 1892 to 1910 I filled the chair of General Chemistry and Physics in the National College of Pharmacy, which, while I was still with it, became affiliated with the George Washington University. From it I received the honorary degree of Doctor of Pharmacy.

BIOGRAPHICAL SKETCH

The foregoing autobiography is obviously incomplete in several respects. It ends with his transfer to the Bureau of Standards, a deficiency which is amended by an excellent bibliography, prepared by Hillebrand himself, which gives a list of his publications down to the end of 1923. Three later titles have been added by the present writer. Another lack is characteristic of Hillebrand's modesty—for nothing is said of the honors he received in recognition of his scientific achievements. Of this, more later. September 6, 1881, Hillebrand married Martha May Westcott, of Perrysburg, Ohio. He died on February 7, 1925, of heart failure, following an operation which, however, was not the cause of his death. His widow and two sons, William Arthur and Harold Newcomb, are still living. William is an electrical engineer at Palo Alto, California; Harold is professor of English in the University of Illinois. The two sons are worthy of their father.

So far we have a clear outline of Hillebrand's ancestry, his education, and his training as a chemist—a training which he received under masters of the first rank, whose teaching was an inspiration. In this he was most fortunate. His career as a chemist may now be considered in four sections, namely, his work in Germany, in Colorado, in the Geological Survey at Washington, and last of all in the Bureau of Standards.

The original work done by Hillebrand in Germany is represented by four published papers. Two of these, in the field of organic chemistry, serve to illustrate the breadth of his training; the other two, the joint work of Hillebrand and Norton, laid the foundation of his reputation. Metallic cerium,

lanthanum, and "didymium" were for the first time isolated in reguline form and their specific heats were determined. This research fixed the valency of the metals and established their position in the classification of the elements. That the "didymium" of 1876 has since been proved to be a mixture of several closely related metals does not detract from the fundamental significance of the investigation.

Hillebrand's career in Colorado began as an assayer in Leadville. We may assume that his work there was done with his usual accuracy, but it added nothing to his permanent reputation. When, however, the United States Geological Survey established a laboratory in Denver, Hillebrand was called to it as a chemist, and his serious work in geochemistry began. To him the appointment was the opportunity of his lifetime, and he surely made the most of it. There were minerals to be analyzed and described, such as the zeolites of Table Mountain and the Cryolite of Pikes Peak, and in this work he had the valuable cooperation of Whitman Cross, the distinguished petrographer. There was also one new mineral to his credit, the ferric tellurite, emmonsite. His analyses of the rocks of Leucite Hills were of more outstanding importance and mark the beginning of an epoch in the history of chemical petrology. The older analyses of igneous rocks had been, as a rule, exceedingly crude; only their more important constituents were determined, while even such common elements as titanium, phosphorus, and manganese were either overlooked or ignored. Hillebrand's analyses were of much higher order, as regards both accuracy and completeness. A new standard was set from which Hillebrand never departed.

In 1885 the Denver laboratory was discontinued and the chemical work of the Geological Survey was concentrated in Washington. Thereafter, for twenty-four years, Hillebrand was my intimate colleague. As Chief Chemist, I was able to relieve him of much purely routine work, to throw opportunity for research in his way, and to encourage him in all his investigations. The work that he published was, however, all his own, and he alone is entitled to credit for it. It naturally followed the same lines as were followed in Denver, but with greater

variety and a much larger field to cover. Rocks and minerals were analyzed and methods of analyses were tested and improved. Incidentally a number of new minerals were recognized and described, and their titles can be found in the bibliography which concludes this memoir. One of them, however, deserves special notice here, as showing Hillebrand's generosity in dealing with colaborers. He was engaged upon a study of the alunite-jarosite group when he received from Prof. S. L. Penfield a sample of supposed jarosite from a new locality. It resembled ordinary jarosite in all external particulars, but proved on analysis to be entirely new. Instead of the potassium of normal jarosite it contained lead, and so was given the name plumbojarosite, and in publishing his description of it he made Penfield, who had had no suspicion of its true character, joint author of his paper. Few men would have shown so much liberality. The men who worked with Hillebrand always received full recognition for their services. Some mines in Utah have in recent years yielded plumbojarosite in quantities sufficient to make it a profitable ore of lead. If it had not been for Hillebrand's discovery of the species, it might have been discarded by the miners as worthless. That actually happened in one instance, until a member of the Geological Survey pointed out the real value of the ore and overcame the skepticism of the too practical miners. We have here a good example of the economic significance of a purely scientific research.

It would be easy to give the mere details of Hillebrand's analytical work, but that would furnish no real evidence of his influence upon other analysts. While in the Washington laboratory of the Geological Survey he made more than 400 *complete* analyses of silicate rocks, on the standard of completeness which he established in Denver. Many more analyses of similar character were made by his associates in the laboratory, following his example and often aided by his guidance. It is not necessary here to discuss technicalities, but the broad significance of this work may be shown statistically as follows:

In his great compilation and classification of analyses of igneous rocks, Washington[1] has tabulated 8,602 analyses pub-

[1] H. S. Washington: U. S. Geological Survey Prof. Paper No. 99, 1917.

lished or made between the years 1884 and 1913, both inclusive. These analyses are divided into two classes, as "superior" and "inferior," a distinction based upon a rigorous system of weighting. Six thousand four hundred and fifty-five analyses are rated as superior, and of these 1088, or one-sixth of the whole number, were made in the laboratories of the Geological Survey. This alone is enough to show Hillebrand's influence upon the work of his immediate associates. The "inferior" analyses of Washington's classification are characterized by incompleteness, bad summation, or by obvious errors in the determination of essential constituents in a rock; and very few such analyses are charged against the chemists of the Survey. Even these analyses are often justifiable. An analysis should be an answer to a definite problem; and if that answer is given by a partial analysis, nothing more is demanded by the petrographer for whom it was made. He states his problem and the answer is given. A complete analysis may be desirable, but it is not necessary.

In 1894 Hillebrand published a paper entitled "A plea for greater completeness in chemical rock analysis."[2] Thus, by precept as well as by example, his influence upon other analysts was widely extended. The analyses of igneous rocks made since Hillebrand began his work have been much more thorough. This improvement is shown in the work of other American chemists, and the admirable analyses made by British and Australian analysts are fairly comparable with his. Petrographers who were formerly satisfied with incomplete analyses now demand something better; and what were thought to be insignificant traces of the minor constituents of a rock are now seen to be useful. The study of igneous rocks is much more exact than when Hillebrand began his labors, and the evidence of the thin section is reinforced by the chemical analyses.

In 1897 Hillebrand wrote a fifty-page introduction to Bulletin No. 148 of the U. S. Geological Survey on the methods of analysis of silicate rocks. The rest of the bulletin was a compilation of the analyses so far made in the Survey. This introduction was soon translated into German and published

[2] Journ. Amer. Chem. Soc., vol. 16, pp. 90-93.

in Germany. In 1900 it was rewritten and enlarged to 114 pages and issued as an independent document, Bulletin No. 176. Another edition, which included the analysis of carbonate rocks, appeared in 1907 as Bulletin No. 305. This also was translated into German. It was republished in 1910 as Bulletin 422, and the series culminated in 1919 as Bulletin No. 700, a book of 285 pages. With each edition Hillebrand's reputation as an analyst was increased and his influence upon both chemists and petrographers was greatly broadened. Other papers on the determination of various elements were published at intervals between the appearance of his bulletins. During the last year of his life Hillebrand was engaged upon the preparation of a more general treatise upon inorganic analysis. This task, unfortunately, he did not live to finish. He left, however, notes sufficient to enable his collaborator, Mr. G. E. F. Lundell, to undertake its completion.

In addition to his analyses of igneous rocks, Hillebrand made many analyses of minerals, and especially of those which contained the rarer elements, such as vanadium, uranium, columbium, tantalum, and the metals of the "rare earths." These anaylses are of the most difficult kind and were made with Hillebrand's characteristic thoroughness and accuracy. He also made important analyses of mine waters and of mineral springs. The spring known as Ojo Caliente, near Taos, N. M., is noteworthy on account of the unusual content of fluorine. A spring water rich in zinc from the zinc region around Joplin, Mo., is also remarkable. Both of these waters were analyzed by Hillebrand.

Among Hillebrand's analyses of minerals, there is one group which has proved to be of extraordinary although unsuspected importance. They led to the discovery of terrestrial helium—an element which was previously known only by its lines in the spectrum of the solar corona. The history of that discovery is briefly as follows:

In the summer of 1887 the present writer obtained from the owner of a feldspar quarry in Connecticut a remarkable crystal of uraninite. It was unusually large, with brilliant luster and apparently of a high degree of purity. It was turned over to

Hillebrand for investigation and a portion of the crystal was broken off for analysis. The first step in the process of analysis was the solution of the powdered material in sulphuric acid, and during solution a slow evolution of gas was noticed. An ordinary routine analyst would have called the gas carbon dioxide and thought no more about it; but this was not Hillebrand's procedure. Carbon dioxide would have been given off rapidly when solution began, but the gas from uraninite was emitted continuously until solution was complete. What was it? In order to answer this question Hillebrand, with the cooperation of Dr. William Hallock, collected some of the gas in a Geissler tube and found it to give a brilliant nitrogen spectrum. Some of it, sparked with hydrogen, formed ammonia, and with oxygen it gave nitric acid. Nitrogen alone of all known gasses fulfilled these conditions, and so it was recorded as nitrogen. This result was so surprising that the investigation was carried further, and samples of uraninite and its varieties were obtained from Norway, Texas, Colorado, and other localities, and *complete* analyses, 18 in all, were made of them. In most of them, but not in all, the evolution of gas was observed; but apart from that fact the series of analyses was by far the most thorough investigation of the composition of uraninite that had ever been made, and it has not since been equaled.[3]

In 1895 Raleigh and Ramsay discovered argon, an essential constituent of the atmosphere, and Ramsay at once began a search for other sources of it. His attention was called to Hillebrand's paper, and from a sample of cleveite (a variety of uraninite from Norway) he isolated the gas and determined its chief properties. A sample of it was sent to Sir William Crookes, who examined it spectroscopically and found and measured the same lines that were given by the helium of the sun. Terrestrial helium was discovered, and since then many other sources of it have been found. At first the discovery was one of purely scientific interest, but helium now is obtained from the natural gas of Kansas and adjacent States in suf-

[3] For Hillebrand's complete paper, see Am. Jour. Sci., ser. 3, vol. 40, 1890, p. 384; also, in U. S. Geol. Survey Bull. No. 78, 1890.

ficient quantities to be used, instead of the inflammable hydrogen, for inflating balloons.

Very soon after the public announcement of Ramsay's discovery I received the following letter from him:

University College, London,
Gower St., W. C., *March* 25, 1895.

My Dear Professor Clarke:

I come to you with a request which I hope you will grant. It is to procure for me a considerable quantity of the uraninite from which Hillebrand obtained his nitrogen. I don't know what to ask for, for I don't know how plentiful the uraninite is. As much as you can conveniently let me have. Of course I will pay expenses. I have obtained from cleveite a mixture of argon and helium! There is hardly any nitrogen, and I am afraid that Hillebrand must have deceived himself. The nitrogen I find amounts to 0.06 a per cent or so of the gas, and may easily have been derived from the water. The gas shows almost all the argon lines—not quite all, suggesting that air argon contains some unknown constituent—and in addition a fair number of lines, some brilliant, some less so, but one of astonishing brilliancy, of wave length 587-49, exactly the wave length of D_3 of the solar corona, which has been ascribed to helium.

A note has been read today at the French Academy, and I have sent in an account to the R. Society. I am off to Paris on Thursday, so I am very hurried. My time is occupied in preparing a lecture for the French Chemical Society. Of course I was led to investigate this gas, so as to obtain more clues to argon compounds, and my next work with argon will be to try the action of uranium oxides on it. But helium is terribly tempting, so I am afraid argon will come second best off for the present.

Kind regards.

Ever sincerely, W. Ramsay.

This letter was referred to Hillebrand, who at once wrote to Ramsay the following letter, of which the original draft is now in my possession:

U. S. Geological Survey,
Washington, D. C., *Apr.* 4, '95.

Prof. Wm. Ramsay.

Dear Sir: A week ago Dr. Hallock, who was so good as to aid me some years since in a certain portion of my examination of the supposed nitrogen from uraninite, wrote me a few lines suggesting the identity of that gas with argon and calling my attention to peculiarities noticed by us at that time in its spectrum. Two days later a New York paper published a telegraphic dispatch announcing your identification of the gas

from cleveite as argon with admixed helium, and today I have been permitted to read your letter on the same subject to Prof. F. W. Clarke. Dr. Hallock's words made comparatively little impression upon me, but since the announcement in the *New York Journal* I have gone over in my mind the whole subject, and even meditated preparing a fresh lot of the gas from some of the Connecticut material, with the intention of having it examined by an expert. In the meantime I waited with interest for further details of your discovery in the hope that the newspaper might not have been altogether correct in the implied statement that the gas contained *no* nitrogen, for while I had about come to the conclusion, for reasons given below, that my gas might well have contained argon, I was very unwilling, and I still am so, to believe that the brilliant spectrum of nitrogen observed by me could have been due to atmospheric nitrogen introduced at some stage of the manipulations. It was this brilliant nitrogen spectrum, coupled with the formation of ammonia and nitric acid, which has led me to believe firmly, notwithstanding certain other facts, that the gas could at most contain no more than small amounts of other constituents than nitrogen. At the same time I have always considered that the gas was well worthy of further examination, and I have often regretted not having at that time turned over some of it to an expert in spectroscopic work.

The circumstances and conditions under which my work in this line was done were unfavorable; the chemical investigations had consumed a vast amount of time and I felt strong scruples about taking more from my regular routine work. I was a novice at spectroscopic work of this kind and was thereby led to attach too little importance to certain observations which in the light of your discoveries deserved the utmost consideration. Among other things, it puzzled me that the formation of ammonia should have proceeded so very slowly, and that only comparatively small amounts of the gas should have been converted at the expiration of even several days' passage of the current. But, having had no previous experience, I was unable to judge whether this was especially abnormal or not; it might have been due to insufficient current or to some other cause. A similar observation applies to the contraction on sparking with oxygen, which was only carried far enough to prove a contraction and to obtain the tests for nitric acid.

It doubtless has appeared incomprehensible to you, in view of the bright argon and other lines noticed by you in the gas from cleveite, that they should have escaped my observation. *They did not.* Both Dr. Hallock and I observed numerous bright lines on one or two occasions, some of which apparently could be accounted for by known elements, as mercury or sulphur from sulphuric acid, but there were others which I could not identify with any mapped lines. The well-known variability in the spectra of some substances under varying conditions of current, degree of evacuation of the tube, led me to ascribe similar causes for

these anomalous appearances and to reject the suggestion, made by one of us in a doubtfully serious spirit, that a new element might be in question. The various degrees of doubt with which the announcement of the discovery of nitrogen in uraninite were at first received were, moreover, little calculated to inspire one with a desire to announce a new element on the basis of such imperfect spectroscopic observations as had been made, and I finally came to the conclusion that the bright lines—since to the best of my recollection they were not constant or always the same in the two or three samples of gas examined—were probably not due to any original constituent of it. For this reason I most unfortunately made no reference in my published paper to an unusual appearance of the spectrum, which I so much the more regret because I have thereby laid myself open to criticism on the score of careless observation.

Do not mistake my purpose in entering thus at length upon explanations. I have not the slightest thought of claiming or hinting at a prior discovery. I merely wish to absolve myself in your mind, in part at least, from the charge of gross carelessness. While the observation was not lacking, unfortunately for me the same can not be said of the interpretation of those observations.

I take pleasure in sending you, under separate cover, by registered mail, the final remnant of that portion of my uraninite from Glastonbury, Connecticut, 3½ grams in weight, capable of affording about 70 c. c. of gas, of which the analysis is given under V in Table I and on which the special experiments detailed on the last few pages of my paper were made. I also inclose three crystals and fragments of crystals from the same locality aggregating considerably more in weight. Their composition is unquestionably the same as that of the 3½ gram lot. I will write at once to a gentleman in Connecticut who was in possession of more of the same material and endeavor to obtain some for you, but am not at all sure of success.

The material in the small tube contains about 2.4 per cent of the gas, but I must warn you that it is a matter of many days to effect anywhere near complete decomposition of this variety by hot dilute sulphuric acid. Fusion with an alkaline carbonate will effect complete liberation in a short time. It is possible that the gas from this variety may really contain nitrogen in some quantity—at least I may be excused for hoping so. Possibly also the gas from bröggerite, which I examined spectroscopically, may be richer in nitrogen than that from cleveite which you examined. True cleveite, as shown by Lindström's original analysis and my No. XVII, differs in composition from bröggerite in some important respects, and is much more readily soluble in acids than bröggerite, just as the latter is vastly more soluble than the Connecticut varieties of uraninite. Hence I can not refrain from suggesting that it were well to examine as many of the varieties as possible in order to ascertain

whether or not the gas differs in composition. If the easily soluble forms have been derived by oxidation from original forms like those of Connecticut, richer in uranium dioxide and gas, as well as much less soluble, it may very well be that the composition of the gas varies quantitatively if not qualitatively. That it is a mixture in one case you seem to have shown.

Above all, it seems to me that a density determination of the gas is needed. The high summations of some of my complete analyses as well as those in which the gas was not estimated (see especially Tables I and II), referred to by me on page 75 of my paper, are *certainly* not to be accounted for by ordinary errors of weighing or by impurities of reagents. I make this assertion with the utmost confidence. They *would* be explained if the gas had a density about half that of nitrogen. While this would appear to be impossible if argon is the chief constituent of the gas, may it not be that argon is really subordinate? Your observation, that the gas from cleveite does not exhibit certain lines which atmospheric argon shows, might be regarded then as confirmatory of this view instead of indicating, as you wrote to Professor Clarke, that atmospheric argon contains something else than argon; for with dilution it is to be expected that certain argon lines would disappear. May not helium, or helium and other elements with densities less than nitrogen and constituting an infra-lithium group, make up the bulk of the gas? It seems not at all an unreasonable supposition that we may have to deal here not with a single element, but with two or more related members of a family or group. If such a specifically lighter gas or gases are present, the percentages given by me must all be reduced to correspond, whereas if argon is the chief constituent they must be largely increased and the summations thus made to depart still more from 100.

I send with this, under separate cover, a copy of my paper to which I have frequently referred, for your greater convenience in case you should have had access to it only in the *Chemical News,* where it appeared in disconnected form, running through several numbers.

My naturally great interest in this matter must be my excuse for writing at such length. I shall await the results of your further researches with the greatest impatience. Should you deem it desirable to make public any part of this letter in connection with your own work, you are fully at liberty to do so. I remain,

Very cordially yours,

W. F. HILLEBRAND.

This letter of Hillebrand's was followed by further correspondence, and the two following letters by Ramsay explain themselves:

12 ARUNDEL GARDENS, W., *April* 21, 1895.

MY DEAR SIR:

I have to thank you most cordially for your very kind and courteous letter of the 4″ April. I never like to have to impeach the work of another; and I am sorry that our results do not agree. But agree they do not. The gas evolved from the sample of cleveite of Norwegian origin which I obtained from a London mineralogist consists of a mixture of hydrogen, argon, and helium; there is no nitrogen. This cleveite is, however, very easily decomposed by weak acid; in half an hour the whole mineral is disintegrated and mostly in solution. I find your sample (for which my warm thanks) much more difficult to decompose. In fact, a quantitative estimation is still in progress. I can tell you nothing as yet about *its* gas.

The density of the original gas from my sample is 1.55 (H-1); and the percentage of hydrogen is

$$\frac{3.7 \times 100}{4.7}$$

roughly. Tomorrow I shall carry out an accurate determination of the amount of H_2. This gives the density of the mixture of He and A as 3.6. I can't guess at the amount of A present, but its lines are distinct enough, in spite of what Cleve says in the Comptes rendus. I am inclined to suspect that the substance evolved is a hydroxide of helium, and possibly a hydroxide of argon; for after metallic palladium has done its best in removing hydrogen, the gas still shows a curious spectrum in which He is not conspicuous, and it is only after explosion and sparking with O that the He and A lines become very marked. There is another odd thing. The gas gives a band spectrum when the tube is filled at a fair pressure (I should judge 10 mm.), and the He and A lines come out strong only when the pressure is considerably reduced.

I hope tomorrow to remove He from a fair quantity of the gas (about 2½ liters) and then to weigh the residue directly instead of calculating its density indirectly. I shall also find the ratio of its sp. hts. tomorrow. I am writing all this in a provisional way; please don't take all that I say as absolutely established—confirmatory experiments must still be made. But, as a first approximation to truth, possibly to be modified later, it may stand as I have written it. It may interest you to know that English pitchblende, of which a good deal is now found in Cornwall, gives very little gas. I haven't examined it yet.

I am anxious to acknowledge your letter and kind present, otherwise I should have delayed a day or two and been able to tell you more about it. I had to give two lectures in Paris at the end of March, immediately after finding He, and my wife wanted me in Scotland for 10 days; so I have just returned to London. This accounts for the apparent small progress made. I have to give a paper on Thursday next, and will

quote from your letter, as you kindly allow me to do. This is the first chance of English publication, for all our societies have been having Easter holidays.

Believe me, yours very truly,

W. RAMSAY.

UNIVERSITY COLLEGE, LONDON,
GOWER ST., W. C., *May* 30, 1895.

DEAR DR. HILLEBRAND:

I have much pleasure in sending you a tube of helium from your own uraninite, and am also glad to tell you that your gas contains a considerable proportion of nitrogen—at least 10 p. c. None of this is visible in the tube I send you, however, for it has all been removed by sparking with oxygen in presence of caustic soda. There was no leakage, so I am very sure that you were right in your original statement. Not all minerals give off nitrogen; so you were exceptionally unfortunate in having lighted on one which does.

Considerable progress has been made with the whole investigation since I wrote you last. The density of the gas from my sample of cleveite was 3.9; but I am beginning to doubt whether it was entirely free from nitrogen, and I am repeating a determination, starting from a fresh sample. My doubts are increased by the different results I get with the gas from bröggerite, of which Professor Brögger of Christiania was kind enough to send me a large stock (120 grams). Its density is 2.2, and I am not yet quite certain whether it is pure as it can be made. However, this last density has been checked several times, and though I shall try again, after a long sparking—the coil is rattling beside me while I write—I have no expectation to change the result. Two at least of the easily visible helium lines in the red are identical with good argon lines, and one in the orange. And this leads to the possibility that what we have called argon may contain one of the constituents of helium. I am at work on this point.

We have now got helium from a great many minerals; almost all of those, but not all, contain uranium. But none of these forms a *source*, except your specimen, the original Norwegian cleveite which I bought here and bröggerite. The quantities are insignificant. I may mention thorite, orangite, samarskite, monazite, as among the richest; but the yield is ridiculously small in all cases.

So far as we have gone, we have been able to make no spectroscopic distinction between helium from all these sources. All lines shine out in all of them and with about equal brilliancy.

An interesting fact is that gas from meteoric iron, after removal by hydrogen and hydrocarbons, consists mainly of argon and the least trace of helium. The argon spectrum is good, but the helium a mere shadow, but an undoubted one. Let me caution you to be economical with the

vacuum tube. The helium spectrum fades on running the tube for long. I suppose the platinum sparked from the electrodes carries it on to the glass. If the tube is run for 5 or 6 hours continuously you will see no more helium. So in exhibiting it to your friends, which you will doubtless do, don't give them more than a minute apiece, or make them learn to look through a spectroscope at something else first, before you turn on the helium. Keep the current as weak as you can to get good definition, and it will last all the longer.

Please give my kind regards to Professor Clarke and assure him that it is only want of time which has delayed my answering his very kind letter. I am in the midst of a commotion and can hardly snatch a moment to write letters.

Yours very truly,

W. RAMSAY.

Hillebrand's accuracy in his identification of nitrogen in the gas from uraninite is thus fully recognized. Hillebrand furnished the clue which led to Ramsay's great discovery. If argon had been known when Hillebrand made his analysis, he might have carried his investigation further and found helium himself. But—

> "Of all sad words of tongue or pen,
> The saddest are these—it might have been."

On July 1, 1909, Hillebrand entered upon his duties as Chief Chemist of the Bureau of Standards. His work in this new position was largely administrative, although he still found some time for original research, in which there was a distinct overlapping from his work in the Survey. He was much interested in the study of minerals containing vanadium, as may be seen from a glance at his bibliography. In 1900 he published a paper upon carnotite and its associated minerals, and in 1913 he described other vanadates from Peru, Colorado, and Utah. The last paper that came from his pen related to the same group of minerals and was published in 1924, not long before his death.

We now come to a line of work which began in the Geological Survey and was afterwards splendidly developed in the Bureau of Standards. Early in 1892 Hillebrand received from the Colorado Scientific Society five samples of an ore containing zinc, which had been a source of trouble to the analysts and

assayers of Colorado. They worked by various short-cut commercial methods for the determination of zinc, and their results, as obtained by different methods, did not agree. The zinc in the samples sent to Hillebrand was determined by Mr. L. G. Eakins, under Hillebrand's direction, and by the use of the slower and more accurate methods employed in the Survey. In this way it was found which of the commercial methods was the most trustworthy, and something was learned as to their sources of error—a great help to the metallurgists of Colorado. In 1905, under the auspices of a special committee of the American Chemical Society, of which Hillebrand was chairman, the subject of the earlier investigation was taken up on a much larger scale. A uniform sample of a zinc ore was prepared and analyzed. A large number of analysts repeated the work, using different and specified methods, and the most accurate procedure was definitely determined.

In 1906 another step was taken in the standardization of analyses. A large sample was prepared of an argillaceous limestone used in the manufacture of Portland cement. This was analyzed by Hillebrand in the Geological Survey, and by Dr. C. E. Waters in the U. S. Bureau of Standards. These concordant analyses were repeated by many other analysts, but with widely variant results; these results were critically compared, their sources of error pointed out, and the cement industry was given greater chemical exactness. Still later three samples of iron were taken, analyzed at the Bureau of Standards, and then distributed to iron masters for comparison with the analyses made by their chemists.

Under Hillebrand's administration as Chief Chemist of the Bureau of Standards, the standardization of analyses was carried much further; and now the Bureau distributes annually about 5000 samples, representing 70 different substances. Of Hillebrand's personal share in this phase of the Bureau's activities his colleague, Dr. Waters, speaks as follows:[4]

"He took the greatest interest in the standard samples and he did everything in his power to increase their number and usefulness. He

[4] See Waters' admirable notice of Hillebrand in *Science* for March 6, 1925.

always jealously guarded the integrity of these samples and would not countenance including among their number any materials the composition of which was not known with an accuracy great enough to satisfy him."

Of Hillebrand as Chief Chemist of the Bureau I can do no better than to quote Dr. Waters, who says:

"He was a kindly chief, ready to discuss the problems and worries of his subordinates, not given to making the facile excuse that he was too busy to talk. He gave to every man his due credit; he sought and obtained promotions in rank and salary for those who, in his opinion, were deserving. Always modest about his own attainments, he gave his associates full credit for whatever the chemistry division of the Bureau accomplished. He took his administrative duties seriously and suffered undue worry lest he should be found wanting. A man in his position must many times make decisions relating to the use of Government funds and be tempted to divert them from their specifically authorized use to some other he may think more worthy. Dr. Hillebrand 'leaned backward' in his uprightness and would not countenance any violation of the letter or the spirit of the law. His honesty in these matters was but a further expression of the integrity of purpose that was the great guiding principle of his life, the principle that made him perform a routine analysis with the greatest care, that made him give to any task the best that was in him."

In 1906 Hillebrand was President of the American Chemical Society. It was a difficult year in the history of the Society, for there were serious dissensions with which the President had to deal. Some of the industrial chemists felt that their interests were slighted; that the Journal of the Society published too little that concerned them, and there was some fear that they might break away and form an independent organization. This danger was averted by the leaders of the Society by establishing a second journal, the *Journal of Industrial and Engineering Chemistry*. A forward step was taken with Hillebrand as the leader.

In addition to the American Chemical Society, Hillebrand was a member of the National Academy of Sciences, the American Philosophical Society, the American Society for Testing Materials, the Washington Academy of Sciences, the Geological Society of Washington, and the Göttingen Gesellschaft, and fellow of the American Association for the Advancement of Science. In 1916 he was awarded the Chandler Medal by

Columbia University. Of this Dr. Waters says: "His address on that occasion, 'Our analytical chemistry and its future,' is well worth reading by any chemist who may be disposed to regard analytical work as uninteresting and not worthy of a man's best effort." That is true of every honestly conducted scientific research, no matter how humble it may be.

LIST OF THE MORE IMPORTANT PUBLICATIONS OF DR. W. F. HILLEBRAND

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BIOGRAPHICAL MEMOIRS
VOLUME XII - THIRD MEMOIR

BIOGRAPHICAL MEMOIR

OF

THEOPHIL MITCHELL PRUDDEN
1849-1924

BY

LUDVIG HEKTOEN

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1925

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In 1898 Prudden wrote: "Twenty-five years ago, pathological laboratories were rare in this country, and such as did exist were usually small corners in the dead-house of some hospital which had more enlightened governors or more money than the rest. In the medical colleges, then largely proprietary, pathology was merged in the chair of the practice of medicine. The student could, if he were enterprising, witness an occasional autopsy, but beyond this his knowledge of this fundamental theme was derived from lectures, charts and books."[1]

In the United States, teaching and research in pathology by men especially trained for the purpose and devoting themselves exclusively to this kind of work may be said to have begun in New York about 1878. In that year, T. Mitchell Prudden was appointed assistant in pathology and director of the laboratory of the alumni association of the College of Physicians and Surgeons in New York. The following year, William H. Welch assumed the professorship of pathological anatomy and general pathology in the Bellevue Hospital Medical College. This was the first full-time appointment of its kind in this country, and the fundamental importance of pathological anatomy in the medical curriculum was now established. Prudden and Welch are the American pioneers in renouncing medical practice and devoting themselves wholly to teaching and investigation in pathology. "Finally there appeared on the horizon in this country a few anomalous individuals who cherished the notion that the science of disease, even in its etiological and morphological aspects alone, was broad and deep enough to command the exclusive attention of its devotees" (Prudden).[2]

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[1] Pathology and the department of pathology. Columbia University Bulletin, No. 19, 1898 (103-119).

[2] Progress and drift in pathology. Med. Record, 57, 1900 (397-405).

It was high time for this departure. After the study of the cell in disease was introduced by Virchow in 1856 (*omnis cellula e cellula*), pathology had developed rapidly. Due in large measure to German influence and example, the need in the medical course of laboratory work in pathology had become acute. It was recognized too that besides clinical and anatomical observation, experimental methods were necessary to progress in research on fundamental pathological problems. And the epochal discoveries of Pasteur and Koch just at this time were bringing in the microbic era with its wonderful progress in knowledge of infection and of prevention and treatment of infectious disease. It was a glorious period for medical science, and in advancing its revolutionary influences on medical education and medicine in this country the subject of this sketch[3] played a leading part.

Theophil Mitchell Prudden was born in the congregational parsonage at Middlebury, Connecticut, July 7, 1849. His mother was Eliza Anne Johnson, 1819-1889, daughter of Eben and Sally Mitchell Johnson at Southbury, Connecticut, and his father was George Peter Prudden, 1819-1872, a congregational clergyman, Yale graduate in arts and divinity, and the direct descendant in the seventh generation of the Reverend Peter Prudden, "among the worthiest of the honored founders of New England," one of the leaders of the New Haven colony in 1638, which he left in 1639 when he established the Milford church. A charming story, privately printed, of the life and work of Peter Prudden in Connecticut has been written by Lillian Eliza Prudden,[4] Dr. Prudden's sister, to whom I am indebted for much information and helpful comment. There were three more sons in the family—Edward Payson, who died in 1843; Henry Johnson, who died in 1890, and Theodore Phi-

[3] The sketch follows closely the biographical notice of Dr. Prudden by Simon Flexner (Science, 1924, 60, 415-419; Annual Report of the National Academy of Sciences, Fiscal Year 1923-24, 1925, 101-109). Since the sketch was written there has been published privately Biographical Sketches and Letters of T. Mitchell Prudden, M. D., edited by Lillian E. Prudden, Yale University Press, New Haven, 1927.

[4] Lillian E. Prudden: Peter Prudden, a story of his life at New Haven and Milford, Conn., with the genealogy of some of his descendants and an appendix containing copies of old wills, records, letters and papers, New Haven, 1901.

lander, M. A. and B. D., Yale; D. D., Illinois College; author of religious books, who died in 1916.

T. Mitchell Prudden had a healthy and happy childhood in Connecticut parsonages with the best of traditions. The parents were guided by "a sympathetic realization of the youngster's point of view and of the necessity for amusement and initiation into knowledge and familiarity with things worth while." Intellectual ideals were implanted early. In adolescence life became more strenuous. The father openly advocated antislavery principles at a time when it was unpopular and even dangerous to do so, and his house was a station on one of the branches of the "underground railway." Ill health soon compelled his retirement from active service, and the later years were spent in New Haven. The eldest son, Henry Johnson, who had literary tastes and eagerly wanted a college education, gave up this wish and entered business in New Haven, where he later prospered. In 1866, T. Mitchell, then seventeen, went to work in his brother's establishment, which he swept and dusted and where he could watch the ways of business, but the duties proved irksome and he gave up the work after about one year. A little later he had a cruising adventure in Long Island Sound in a fishing schooner that ended in mutiny of the crew, landing of the passengers on the rocks of Stratford Light, and his return to New Haven on foot. In the meantime with the aid of his brother, he had made a start toward college and science.

In addition to the schooling he had received in various places, he prepared for Yale at Wilbraham Academy in Massachusetts and entered the Sheffield Scientific School in 1869 under a State fellowship with free tuition. At the end of the first year, medicine was settled on as the goal. Here was the chosen field in which he might hope to be able to satisfy his high ideals of life and service. As yet no provision had been made in the Sheffield Scientific School for instruction in zoology, botany, organic and physiological chemistry. Realizing the importance of these subjects for medicine, Prudden and his friend and classmate, Thomas Russell, later professor of surgery in Yale Medical School, appealed to the faculty, and "the biological course in preparation for medicine" was established. There is good reason to believe that this appeal for a biological course was inspired

by the enthusiasm and insistence of Prudden. The Yale College catalogue for 1870-71 contains this statement about the course preparatory to medicine: "During one year the work of this course will be chiefly under the direction of the instructors in chemistry; during the second year, under that of the instructors in zoology and botany. In chemistry, especial attention will be given to the examination of urine and the testing of drugs and poisons; in zoology to comparative anatomy, reproduction, embryology, the laws of hereditary descent and human parasites; and in botany to a general knowledge of structural and physiological botany, and to medicinal, food-producing, and poisonous plants. The studies of the select course in physical geography, history, English literature, etc., are followed by these students." Prudden and Russell were the first two students in the biological course. They "were invited for special advanced work in botany into Eaton's Herbarium at his house; they were placed in Johnson's private laboratory for physiological chemistry; they worked rather as assistants than as students under the eye . . . of Verrill and Sid. Smith in the old bug lab. (alias zoology laboratory)." It may be recalled here that a laboratory for physiological chemistry was organized as an integral part of the biological course by R. H. Chittenden in 1874.

Beside his association with Thomas Russell, Prudden became allied intimately in scientific interests with James Thacher (1847-1891), tutor in physics and later in zoology at Yale, investigator of vertebrate involution, and from 1879 professor in the medical school, to the development of which he devoted himself with marked success. The last two years in Sheffield were full of action—collecting of animals and plants on land and water about New Haven, courses with Whitney, Lounsbury (honorable mention in English composition), and Gilman, membership in Berzelius ("invited early to join Berzelius, its associations were throughout among the most salutary of the college influences"), scientific editorship of *Yale Lit.* He received a prize in minerology. In the spring vacation of 1872 Prudden and Russell chartered a yacht and with a few choice spirits made a dredging expedition down the Sound. They brought back much valuable material and established new habi-

tats for several marine vertebrates. Theirs was the first dredging about Woods Hole.

In 1872, Prudden graduated as A.B. and entered the Yale Medical School without delay. As illustrating the confidence his teachers felt in him, it may be mentioned that at this time he taught elementary chemistry, and with noteworthy success, during the absence in Europe of Professor Mixter of the Sheffield Scientific School. At the same time he served as secretary of the faculty. He received his medical degree in 1875. In the spring of that year he spent some time in New York in studying pathology with Dr. Francis Delafield at the College of Physicians and Surgeons, a visit that undoubtedly came to be of great influence in his further career. Companions since Sheffield days, he and Thomas Russell now served together as internes in the New Haven Hospital, six months each on the medical and surgical services. At this time this as well as practically all other hospitals in this country had no facilities for any kind of laboratory work for clinical or other purposes, and it is greatly to the credit of Prudden's interest and vision that he arranged a small laboratory in a basement room of the hospital. His interest in fundamental medical problems had survived the didactic grind and lack of contact with realities of the medical course of that day. On completion of his internship, Prudden at once went to Germany to work in pathology under Professor Julius Arnold at Heidelberg. This step no doubt was planned long in advance and probably had the cordial support of Dr. Delafield and other advisers. In a letter of September 10, 1876, Arnold writes that a place has been reserved and that the only fee required is 10 marks for registration. In the winter of 1876-7 Prudden followed the lectures and laboratory courses of Arnold and his associate Thoma and worked on changes in living cartilage. Later he visted other centers, including Vienna, and worked in other laboratories, returning to New Haven after an absence of two years. He now had mastered German and established friendly relations with several of the leading workers in pathology. "When most of us were serving our novitiate in pathology, the study of inflammation was largely limited to a bare description of visible phenomena and a cataloguing and classification of lesions . . .

the more inquisitive among us were much exercised to find out whether it was the emigrated leucocytes or the fixed connective tissue cells which were most concerned in the formation of new cells. So earnest were the advocates of each of these views that the social amenities sometimes suffered. Thus it was my hap to be banished from Stricker's laboratory in Vienna when it became known to that champion of the connective-tissue cells that I had been under the baleful influence of Cohnheim and Arnold."[5] The result of his research under Arnold was published in Virchow's Archiv in 1879 (see Bibliography). The work deals with changes in living cartilage, a subject of special interest now in the day of vital staining and in vitro study of animal cells. He proves to have been endowed with a high grade of investigative workmanship. An effort was made to follow the effects of harmful agencies on living cells under otherwise normal environment. By a clever device the transparent episternal cartilage of the frog was observed for hours under the microscope while connected with the body. The chromatin of the nucleus was recognized, variations in cell form and content were produced at will, and the important fact was noted that certain dye solutions do not color living cells but do color dead cells. "But it seems to me particularly significant that it is possible to observe under the microscope, on living cartilage tissue, the processes of contraction and the formation of vacuoles; and that it is possible thus to determine whether and under what conditions such changed cells return to the normal state or whether they undergo degeneration and die. The observations made with reference to the behavior of living and dead cartilage cells in response to dyes also seem to me noteworthy, because we thus learn that only the nuclei of the latter stain homogeneously. We are, therefore, in a position to distinguish whether cells are dead or living, and can thus exclude their participation in regenerative processes." It is a spirited, pioneer study of what actually goes on in living cells as distinguished from inferences drawn from the appearances of cells in dead tissues fixed by chemicals and stained.

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[5] Progress and drift in pathology, Med. Record, 57, 1900 (397-405).

The illustrations were drawn by himself. His later work in New York on cartilage transplantation and on the effects of carbolic and salicylic acids on ciliated cells, leukocytes and vascular membranes is a direct continuation of the study in Arnold's laboratory.

When Prudden returned from Europe, he was eager to introduce into the medical course laboratory work in pathology and to devote himself to teaching and research. In order to be able to do so he had made up his mind after much questioning to give up for good the thought of practicing medicine. He hoped he might have the good fortune to become a teacher of pathology on full time. As stated, in those days in this country the teaching of pathology was done by the professor of the practice of medicine, the exception being Harvard Medical School in which J. B. S. Jackson and Reginald H. Fitz taught pathology, and the authorities of several medical schools and certain leading physicians with whom Prudden discussed the situation, did not seem to take any special interest in his plan. Having failed to find a place to work in pathology even as a volunteer, he reluctantly opened an office for practice in New Haven and accepted a lectureship in histology in the Yale Medical School. While the medical schools may have seemed satisfied with the situation as it was, a new movement nevertheless was under way. Among the most influential real leaders in this movement were Francis Delafield (1841-1915, Yale, 1860), a great medical teacher and student of pathological anatomy, and Edward Gamaliel Janeway (1841-1911), one of "the foremost clinical teachers and consultants of his generation." These two remarkable men and the brilliant J. W. Southack, who died young, conducted systematic autopsies for the first time in New York City.[6] Through their work in the dead-house these men and others such as Reginald H. Fitz in Boston, and Christian Fenger in Chicago, came to learn the changes of disease as the greatest clinicians knew them in the past, and as few will ever know them in the future, now that pathology is a separate field of investigation. The following extract from the remarks of Dr. William H. Welch,[7] at a dinner in his honor, are of special

[6] Theodore C. Janeway in American Medical Biographies, 1920, 610-14.
[7] In Honor of William H. Welch, Baltimore, 1910, p. 41.

interest at this point: "Dr. Delafield little knew, when he used to give that summer course in pathological anatomy in the College of Physicians and Surgeons of the interest which he stimulated in me at that time. When he came to know me better he honored me—and I felt at that time it was the greatest honor that could come to me—by giving me the privilege, in his absence, of making an autopsy at Bellevue Hospital and then permitting me to record the protocol in his own private autopsy book. He little knew the seed he was sowing then, for this certainly was the very beginning of my interest in pathological anatomy."

In 1876, Delafield was made adjunct professor of pathology and the practice of medicine in the College of Physicians and Surgeons in New York (full professor in 1882). In 1877, a fund of $10,000 was raised among the alumni of the college "for advancing the standard of medicine" there, which on the advice of Delafield was devoted to a histological and pathological laboratory, and soon after Prudden's setting up an office in New Haven, Delafield offered him the position of assistant and director of the laboratory. After careful consideration, a letter was written in which this offer, which brought a turning point, was declined with regret because financial and other circumstances did not justify cutting adrift from old associates and the modest living as practitioner and teacher in New Haven. The letter was to be posted the next morning, but during the night the spirit of devotion to science and of adventure caused a change of mind, and the offer was accepted. This momentous decision, which was followed by a career of forty-two years of teaching and investigation and of leadership in medical progress, was communicated promptly to Professor Arnold in Heidelberg, and in a letter from him dated November 15, 1878, occurs this passage: "Your letter pleased me greatly, because I am convinced that the position you have assumed will assure you a future. From my knowledge of you, I am, I believe, justified in saying that you will not now easily be diverted from the goal of a scientific career. There is no doubt in my mind that your institute will be a success. The fact that it is small and has only moderate means at its disposal is not important. Some time ago, our German pathological in-

stitutes, with the exception of a few, were very bad; now they are being replaced by good ones. But I know from personal experience it is possible to work in even a small institute."

The new laboratory of the College of Physicians and Surgeons was supported largely by the alumni association of the college, the constantly recurring deficits being made up by Dr. Delafield. Evidently part of the money for running expenses was obtained for a time by Dr. Delafield from student fees. The work started with rather meagerly attended voluntary classes in normal histology. Before long, the number of students began to increase. In the meantime, Prudden assisted Dr. Delafield in autopsies at the Roosevelt Hospital and became a skilled and careful pathological anatomist. Dr. Prudden himself describes the first laboratory as follows: "It was a narrow store on the ground floor, on Fourth Avenue, with a scanty strip of sky just visible through an iron grating, and with scarcely a feature adapting it to the needs of a microscopic laboratory, save that its walls kept out the wind and rain. An ice-cream store on one side and a harness shop on the other; the clatter of wagons and horse-cars and pedestrians sweeping endlessly along the street in front; the small boy peering curiously between the iron bars of the windows at the strange performances within, linked science to the busy world in a fashion truly cosmopolitan. The great brewery wagons rumbling heavily along the pavement set every microscope a-tremble; and the frequency with which microscopic observation must for this reason be suspended, while a severe strain upon the temper of the devotee to science, often left him free to muse upon the important rôle which beer plays in modern metropolitan life." [8]

Before long, Professor Arnold's prophecy began to come true. The curriculum was extended, more laboratory work was required, a stimulating atmosphere prevailed, and the lean years came to an end when the college moved into the new building on West 59th Street with a splendid, in fact long unequalled, group of laboratories. Prudden continued as first assistant and direc-

[8] Pathology and the department of pathology. Columbia University Bulletin, No. 19, 1898 (103-119).

tor of the laboratories from 1872 to 1892. He gradually attached to his laboratory a growing group of enthusiastic workers in whom developed the spirit of research and teaching. In addition to their academic duties, some of the assistants acted as pathologists to hospitals, dispensaries and physicians, the compensation for this service being specimens for teaching. During these years, Prudden took an efficient and tactful part in the administrative affairs of the institution. When the college at last became in fact a part of Columbia University in 1892, a department of pathology was created of which he was professor until he retired as emeritus in 1909.

From the beginning of his directorship, normal histology was associated with pathology, largely for convenience in administration. For two or three years after coming to New York, Prudden seems to have taught normal histology at Yale also. It consequently is not strange then that his first publication in New York should be "Notes on the Course in Normal Histology," which in two years were expanded into his "Manual of Practical Histology," five editions of which were issued, the last, revised by his assistant, G. C. Freeborn, in 1893. Soon articles began to appear on results of experiments on cells, on observations on a wide range of topics in pathological anatomy, and on the tubercle bacillus and other bacteriological subjects. At this time there was no textbook in English that could be said to do justice to pathological anatomy as studied and taught especially in Germany. Even before his return from Europe, Prudden considered translating a small book by Perls, but he was unable to obtain an American publisher. The need in question was filled by the publication in 1885 of a second edition called "Hand-book of Pathological Anatomy and Histology," by Delafield and himself, of Delafield's "Hand-book of Post-mortem Examination and Morbid Anatomy" (1872). A new edition was called for in less than one year. This book still holds its place as a standard text. The subsequent editions all were revised with special care and each was more complete and comprehensive than its predecessors. The number of illustrations, most of them drawn by Prudden himself, increased regularly. With the sixth edition in 1901, the main title was changed to a "Textbook of Pathology, etc." Dr. Delafield now

no longer shared actively in the revisions. In the preface to this edition it is stated that it has seemed wise now to dwell on the relationships of pathology to the allied phases of biological science and "to view pathology as one aspect of the diverse manifestations of life and of energy, rather than as belonging to a special and exclusively human domain." The eleventh edition (1919) was revised by Francis Carter Wood, one of Prudden's pupils. The twelfth edition appeared in 1922, the thirteenth in 1925 and the fourteenth in 1927. The success of the book rests on the clear and precise grasp of the principles as well as the practical details of pathology and on the polished and lucid language.

The epochal work of Pasteur and Koch in microbiology met with a quick response from Prudden, who promptly grasped the significant part the new science would play in medicine, and as soon as the announcements of the discoveries in infectious diseases began to stir the medical world, he partitioned off for bacteriology a small corner of his dark and crowded laboratory, "with second-hand glass sashes—the wreckage of a livery stable." "So small was this apartment, that the worker standing at his table with its twilight illumination, could touch the walls in all directions, while at frequent intervals he must beat a hasty retreat for a breath of fresh air." This was one of the earliest bacteriological laboratories in this country.

Koch reported the discovery of the tubercle bacillus, March 24, 1882, and in 1883 Prudden published two articles on its demonstration in tuberculous lesions. He soon planned to return to Germany to work if possible with Koch. To this end he enlisted the good offices of his teacher, Professor Arnold, who sent a note from Koch in which he says that "at the moment it is not possible to accede to the request of Dr. Prudden, as the few available places at Gesundheitsamt have already been assigned for some time ahead." Koch adds: "The Hygienic Laboratory will be open within the next few months and I expect to give a bacteriological course there." Arnold suggested a preliminary course with Hueppe in Wiesbaden or Frobenius in Munich. Prudden's desire to work under Koch was realized; in May, 1885, he was requested by the Connecticut Board of Health to make a report on the application to health

work of recent developments in bacteriology, and accordingly he spent two months in Koch's and Hueppe's laboratories. In his report of the course he followed in Koch's laboratory, he gives his impressions of Koch himself: "—the calm, judicial mind of Dr. Koch—the master worker in his field—his marvellous skill and patience as an experimenter, his wide range of knowledge and his modest, unassuming presentation of his views, are all calculated to inspire confidence in the results of his own work, to stimulate his students to personal exertion in this field, and to lend certainty to the already widespread hope that ere long through the resources of science we shall be able to cope successfully with those most terrible and fatal enemies of the human race—the acute infectious diseases."[9] In his report he urges the appointment of a bacteriologist by the board and the establishment of a laboratory for examination of water, milk, food, and for research, on a plan similar to that of agricultural experiment stations, and he suggests that possibly the laboratory might be connected with some existing institution like a medical school. The purpose of the laboratory was to serve as a center of control and research. This surely is one of the earliest definite plans for a bacteriological laboratory in connection with a board of health.

In the same year of 1885, Timothy Matlack Cheesman began to give instruction in bacteriology in Prudden's laboratory, and as stated, research was begun even earlier. Prudden was the main instrument through which the new knowledge of bacteria was brought into New York. He made this knowledge practically potent through his influence with the city health officials, particularly, Herman M. Biggs, his report to the Connecticut board and by a well-planned newspaper correspondence carried out anonymously over many years, as well as by his researches and instructive popular writings on the relations of water and ice supplies and dust to health and disease. His booklets, "The Story of the Bacteria" (three editions), "Dust and Its Dangers" (two editions), "Drinking-water and Ice Supplies and their

[9] On Koch's method of studying the bacteria, particularly those causing Asiatic cholera. Eighth Annual Report of the State Board of Health of the State of Connecticut for the year ending Nov. 1, 1885. 1886 (213-230).

Relations to Health and Disease" (two editions), carried welcome and timely information to a much wider circle of readers than merely the medical group. Their style is simple and delightful, they were read eagerly, and served effectively in spreading the knowledge on which public health, hygiene, and the modern diagnosis and treatment of infectious diseases are based. Particular mention must be made of his work on the bacteriology of ice because of its importance as the pioneer groundwork that insures the supply of sanitary ice. He made extensive studies of the bacteriology of ice and demonstrated the necessity of the control of ice fields and of artificial ice, thus contributing largely to the solution of the ice problem.

It was no accident that led the department of health of New York City to become one of the first to apply bacteriological teachings to practical public health work. The report by Prudden, Biggs and Loomis, pathologists, to the board, on the nature and mode of spread of infection in tuberculosis, and the memorandum of its acceptance in 1889, constitute a landmark and a point of departure in the campaign against tuberculosis. The report, recently reprinted because of its historical importance,[10] is a brief and comprehensive statement of principles then new, the crucial point being that "tuberculosis can be prevented—it is not directly inherited and is contagious." Of this report, 10,000 copies were distributed. In 1892, Herman Biggs, whom Prudden characterized as "wise in counsel, ready in service, a good comrade," became director of the newly organized diagnostic laboratory of the health department of New York City, one of the earliest of this new kind of laboratory.

In the meantime, more and more attention was devoted to bacteriology by Prudden and his co-workers. Important details in various infectious diseases were worked out. With his assistant, Eugene Hodenpyl, the genesis of the lesions of tuberculosis was studied extensively, and they were able to show that the tissue may react to the presence of dead tubercle bacilli by forming typical tubercles. Prudden further demonstrated that cavities may develop in the lungs of rabbits by injecting tubercle bacilli through the trachea into the lungs, where they would set up a caseous bronchopneumonia, and then injecting streptococci

[10] Health News, 17, 1922 (89).

later. He produced malignant endocarditis experimentally and studied bacteriologically the etiology of pleuritis and influenzal pneumonia.

Soon after the announcement of the discovery of the bacillus of diphtheria, Prudden in 1889 published the results of a careful bacteriologic study of 24 asylum and hospital children with throat infections resembling diphtheria. Only streptococci were found and those in such numbers that he regarded them as the causative factor in the group of cases he had studied. Subsequently it was found that these cases taken as a series should not have been regarded as typical instances of primary diphtheria, but rather as a mixed group of membranous throat infections secondary to measles, scarlet fever and other diseases. The momentary confusion in regard to the causative rôle of the bacillus of diphtheria that resulted from Prudden's report was dispelled completely two years later when he published the results of further studies of diphtheria that led him to the conclusion that the name diphtheria should be applied exclusively "to that acute infectious disease, usually associated with a pseudomembranous inflammation of the mucous membrane, which is primarily caused by the bacillus called *Bacillus diphtheriæ* by Löffler." In the meantime he had found, with his lifelong friend, W. P. Northrup, that in seventeen children dying from diphtheria complicated with pneumonia, streptococci were present in the lungs of all save one. The results of this work on diphtheria served to make clear the dangerous rôle of streptococci as secondary invaders. Prudden also dipped into immunology and was one of the first to study the germicidal action of serum.

During all these years, hundreds of medical students later going into the various lines of medical service, came under the immediate influence of his high ideals of life and scholarship. Dr. E. O. Jordan, professor in the University of Chicago, writes: "I spent the month of October, 1888, in Dr. Prudden's laboratory. Dr. T. M. Cheesman and Dr. G. C. Freeborn were assistants at the time. Dr. Prudden's energetic, genial personality dominated the whole place. Although my knowledge of bacteriology was exceedingly rudimentary, he gave freely of his time and advice. He told me briefly what to do, then turned

me loose and after a few days, probably once a week, discussed results. It was a very stimulating and fruitful experience, and I have profited all my life from even that brief contact." One of Prudden's assistants writes: "As a teacher he was an inspiration to the keen and serious student and a sarcastic enigma to the dullard. In his lectures, which were charmingly delivered, he probably at times addressed the upper half of the class. He was very clear in the exposition of essentials. He took keen delight in the fine points of demonstrations, and this attitude was communicated to the students. . . . As a medical educator he was for many years the main factor in the development of the College of Physicians and Surgeons, acting through James W. McLane (the dean). For a long time these two had weekly and even daily conferences." Besides instruction of medical students in normal histology, bacteriology and pathology, the department offered facilities for advanced work, particularly to physicians and candidates for higher degrees. In 1898, Prudden wrote that the number of advanced workers each year was about twenty. At that time a special library was supported by the workers in the department, and about seventy-five periodicals were received regularly. From 1890, volumes of reprints of the most important articles published from Prudden's laboratory were issued under the title of "Studies from the Pathological Laboratory (since 1893, Department of Pathology) of the College of Physicians and Surgeons."* At the time of Prudden's retirement, eleven substantial volumes had

* The following lists which are not claimed to be complete, contain the names of assistants in Dr. Prudden's laboratory—Bacteriology: Timothy M. Cheesman, Harrison G. Dyar, Rowland G. Freeman, Philip Hanson Hiss, Clinton B. Knapp, Charles Norris; histology: James Ewing, George C. Freeborn, Edw. M. Kitchell, Ira T. Van Gilson; pathology: Frederick K. Bailey, John H. Borden, Hughes Dayton, John S. Ely, James Ewing, Eugene Hovenpyl, D. S. D. Jessup, John H. Larkin, Augustus J. Lartigau, Charles Norris, Wm. Hallock Park, Augustus Wadsworth, Francis Carter Wood; photography: Edw. Leaming.

This list gives the names of the volunteer workers represented by articles in Studies I-XI: James P. Atkinson, Pierce Bailey, William H. Baker, William H. Bates, David Bovaird, John W. Brannan, William C. Clark, Edw. B. Coburn, R. Denig, Charles N. Dowd, Haven Emerson, Elmer W. Firth, Charles B. Fitzpatrick, Robert T. Frank, Henry Heiman, Henry A. Higley, Luther Emmet Holt, Smith Ely Jelliffe, Arnold Knapp, Gustav Langman, Isaac Levin, Samuel J. Meltzer, Alexis V. Mashchowitz, Matthias Nicoll, Charles C. Ransom, Frederick F. Russell (U. S. Army), Thomas C. Southworth, William J. Stone, R. W. Taylor.

appeared—an impressive record. He watched over the work in his laboratory with extreme faithfulness and subjected the reports and papers of his associates to the high literary and scientific standards that he applied religiously to his own productions.

But Prudden was not only a leading pioneer teacher and worker in pathology and bacteriology—his articles and books on American archeology reveal him as one of the elect in that science also. Many summers he spent in travel, chiefly in the southwestern parts of the United States. A disinclination to mingle with people generally has been mentioned as one reason for spending his vacations as he did. He hunted fossils with O. C. Marsh in Nebraska one summer, and for eight summers he wandered with pack train over Colorado, Utah, Arizona and New Mexico locating prehistoric ruins. It is doubtful if any one else ever explored so thoroughly the difficult region of the arid plateaus of the San Juan country. He brought back maps of the canyon systems, notes on the climate, and descriptions and photographs of the hundreds of ruined pueblos and cliff houses that he found. His interest lay in the fundamental problems of culture growth. The early basket-making culture was first described by him. His article on the prehistoric ruins of the San Juan watersheds (1903), is said to be a model report with a most valuable map and especially important because it includes his identification of the old "unit-type" of pueblo structure, and thus lays the foundation for all subsequent research on the developmental side of southwestern civilization. This discovery led him to excavate a number of villages. He gives a vivid and delightful account of the southwestern and desert travel in his "The Great American Plateau." His kindness endeared him to all with whom he came in contact; to be known as a friend of his was the best credential on the Navajo Reservation. In later years, when ill health kept him from his beloved San Juan country, he generously and quietly, as was his wont, contributed to funds for expeditions, and gave advice and friendly criticism of great help to the younger men entering the field of southwestern archeology.[11]

[11] The statements about Prudden's work as archeologist are gathered from American Anthropologist, 1925, 27, p. 149.

His very valuable collection of pottery was presented to Yale University, and models of the early cave dwellings are in the American Museum of Natural History. Of his writing and of his kindly relations with the Indians, a good example is the introduction of his address to the graduating class of Yale Medical School, June 25, 1895:

"I wandered last summer over that marvelous land of sunshine in our great Southwest where still fast dwindling groups of the real Americans cherish quaint customs, and linger among the superstitions of vanished centuries. And Fortune made me for a time a guest in a small tribe of these Indians, as yet almost untouched by the blighting finger of what to us is civilization.

"I was drawn to them in this way: There came to our camp upon the plains, one evening, a woebegone dark fellow of this tribe, who with his squaw had wandered away from his comrades, seeking a quiet place to die. He was wan and feeble. A demon, he told us, had long since gained entrance to his body and had tortured him with pain and cold and fire. All the art of his tribal medicine men had failed to free him from the intruder, and a little while before, some spirit had begun to whisper to him in his sleep, he said, that he must go into the dark. All this was gathered from lip and gesture and pantomime as he lingered with us, loath to relinquish at the last scant comfort of human companionship.

"In the light of the lore which had been imparted to me many years ago by my medical teachers here at Yale, I reckoned him the victim of malaria; and shortly, in fact, quinine had cured him. The demon was exorcised, the spirit ceased to whimper, the sun was again his friend, and the winds began anew to breathe to him their wonted biddings to the chase. The grateful soul, now eager to be away, was urgent that I should visit his people, for he was fain to celebrate the facility of the white medicine man who could banish evil spirits without rattle, dance or chant. And so I went. Eighty miles across the desert from any settlement, down at the bottom of a rock-walled chasm which leads into the Grand Canon of the Colorado, and whose sides tower sheer half a mile, these brown-painted folks have lived alone and almost forgotten since long before the Spanish pioneers came hither for God and gold some centuries ago.

"At the time of my visit several persons in the tribe were ill, and a celebrated medicine man had been summoned from afar in council over the stricken ones. After long and repeated conferences, my dark medical brothers consented to lay aside cherished forms of professional etiquette and permitted me to take a place in the grave circle which at midnight crouched about a small fire built in the open, near which lay, half naked, the group of patients. One of these was clearly the prey of consumption; one was shivering with malarial poisoning; one was a croupy child; one I judged to have partaken unwisely and too much of spoiled jerked meat; and one was the victim of old age. I have not time to picture for you, as I should like to do, the weird scene which was enacted there from midnight until dawn, night after night. A low rhythmic chant rising and falling to the time of a rattled gourd; slow passes of the hands over the prostrate bodies, which now and again were blown upon from the pursed lips of the painted Aesculapius, who now crouched crooning beside his charges, now danced furiously about them, while at frequent intervals wild yells from the attending circle woke hideous echoes from the cliffs. I will not dwell upon the sequels of this adventure, but remind you only that the conceptions of disease which these people foster, and the practices which they adopt to free the body from what is to them a definite possession by some evil thing, are essentially those which were prevalent ages ago, and whose significance we glean so toilsomely today out of the misty and broken records of the past."

Prudden was one of the advisors consulted in regard to the founding of the Rockefeller Institute for Medical Research, and became an original member of its scientific board. He was first vice-president of the board and first chairman of its executive committee, holding these offices until he died. He refused to become president of the board, although he was urged repeatedly to accept the office. His services to the Institute are stated to have been invaluable. He served also on the International Health Board and on the Public Health Council of the State of New York. In 1897 he received the honor of the degree of LL. D. from Yale. Prudden became a member of

the Academy in 1901. He was an honorary member of the Connecticut State Medical Society. He was averse to joining more societies than compelled to by necessity and had no hankering for honors. He served as president of both the New York Pathological Society and the Practitioners' Society of New York, and was a member of the Association of American Physicians, the New York Academy of Medicine, the American Association of Pathologists and Bacteriologists, and the Society of American Bacteriologists. His avocational interests are illustrated by his memberships in the American Anthropological Association, the American Ethnological Association, the Archæological Institute of America, the New York Historical Society, the Sequora League, and the American Folk-Lore Society.

Soon after graduation he withdrew from the Congregational Church and he did not ally himself with any church again. There was no antireligious element in this action; he simply did not wish to be obliged to subscribe to matters of which he was not quite sure.

Dr. Prudden was not married. Always a most agreeable host, he curtailed his social contacts to save his strength for his work and seldom accepted even the most alluring invitations. He was a member of the University Club, the Century Club, and the New York Athletic Club. Dr. James Ewing, pupil and assistant, writes: "Dr. T. Mitchell Prudden was a gentleman of distinction in personal habits, social relations and intellectual pursuits. He was of tall, slim build and walked with a lively, rapid step. His apparel was plain and well chosen. He lived in a spacious apartment, overlooking Central Park, presenting an urban view that in beauty and attractiveness could hardly be equalled in any other city . His tastes were esthetic in all respects, and his desires moderate and fully controlled. He had comparatively few intimate friends and was not easy at first to approach, but he was intensely loyal to the chosen few. . . . He had no questionable habits and nothing could distract him from his work. His conversation was couched always in refined, moderate language. He chose all his words carefully and was a most careful student of language, making fine distinctions in the use of words, a habit which translated itself into fine distinctions in judgment and action. He recog-

nized shortcomings in the medical profession, discussing them quietly and with regret rather than asperity. Overexertion and overindulgence of every type he spared himself, but he seemed never to waste a moment. . . . Instinctively retiring, he preferred to keep in the background." But that Prudden was not afraid on occasion to stand squarely athwart the current is illustrated by his defense of the quarantine administration of Dr. Alvah H. Doty, whose displacement "under cover of a mock investigation and a prodigious defamatory hullabaloo" he condemned fiercely. He recognized fully the importance of the legal applications of medical knowledge and urged the promotion of medicolegal science. In his earlier years he occasionally served as expert, but he found it impossible to continue such work.

Prudden retired from his professorship in 1909, but continued to take an active interest in medical research and public health work until the last. In his later years he was in poor health, but he bore illness and disability with remarkable equanimity and wrote in charming fashion for general readers on tuberculosis and its prevention, on yellow fever, on the new outlook in the conquest of disease through chemotherapy, and other topics. He also paid earnest and impressive tribute to the lifework of his friends and associates, Herman M. Biggs and Luther Emmett Holt. He went to the Rockefeller Institute daily. Tuesday morning, April 8, 1924, he attended a meeting of the Public Health Council of the State of New York and the next morning he was at the Rockefeller Institute, intellectually alert and active as ever. That night he died of coronary thrombosis.

Prudden's initial and personal equipment appears to have been the essential stimulus that directed his life. Ambition for service in science manifested itself early. He looked on the facts with level eyes, indulged not in loose speculation, and loved the lucid honesty of the scholar. Apparently austere, he had a heart of rare warmth and richness. The forty-two years in New York were years of splendid and unostentatious service. He was a pioneer leader in pathology and bacteriology. For many years he was the central figure in the scientific medical life of New York City. In developing his department of teach-

ing and research he revealed an exceptional administrative ability which is seen also in the success with which he served other scientific and philanthropic enterprises in their formative stages. He aided greatly indeed in the development of the health departments of both city and State to their present efficiency, and through research and writing he took a most effective part in promoting the public health movement. His archeological work was admirable. And, crowning all, he had the gift of inspiring in others zeal for high accomplishment in seeking new truths.

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BIOGRAPHICAL MEMOIR

OF

ERNEST FOX NICHOLS
1869-1924

BY

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PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1925

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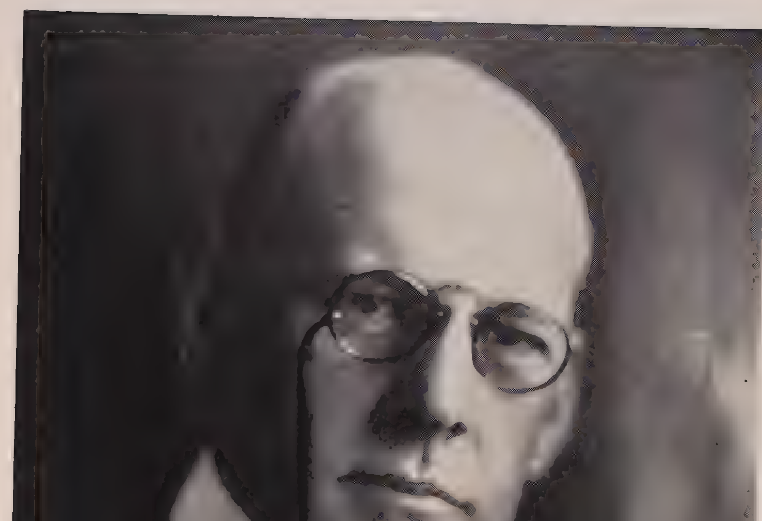

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One winter evening in the year 1885 the present writer lectured at the Kansas Agricultural College. It was an illustrated talk on experimental physics to students who thronged the college chapel. Some three years later, when that event had passed into the realm of things half forgotten, two young men appeared at the physical laboratory of Cornell University. They explained that they had been in the audience at Manhattan on the occasion just mentioned and had been so strongly interested that they had decided then and there to devote themselves to the study of physics. Now, having finished their undergraduate course they had come east to enter our graduate school. One of these two Kansas boys, both of whom were then quite unknown to the writer, was Ernest Fox Nichols.

Nichols was born in Leavenworth, Kansas, on June 1, 1869. He was soon left parentless, a lonely boy but with means to help him obtain an education, and went to live with an uncle and aunt, Mr. and Mrs. Fox, of Manhattan, in that State. He was tall, fair, clear-eyed, of open countenance and winning smile, and there was that about him which once seen was never forgotten. Citizens of Ithaca who met him casually while he was a student more than thirty-five years ago, still remember him with fondness as one recalls some very special friend.

The scientific activities of Ernest Fox Nichols fall into several well-marked periods separated by intervals of more or less complete inactivity so far as published research was concerned. These gaps, some of which were of considerable duration, were by no means times of idleness. It was rather that other duties and interests, always more or less urgent, then became dominant. Each period of activity had a new theme, a *Leitmotif,* and in this way, rather than by the intervening breaks, they are to be distinguished.

The Cornell Period (1888-92)

This period was devoted chiefly to training and to gaining an acquaintance with the great scientific domain of radiation in which his lifework was to lie. The experimental investigations undertaken at this time were suggested primarily for the purpose of giving practical experience in the technique of research. While they did not have the fundamental importance of some that were to follow they afforded results of more than passing interest.

The first of these researches was a study of the absorption of infra red radiation in various optical media. The instruments were a spectrometer, with a linear thermopile in the eyepiece, and a galvanometer. To make measurements even in the more intense regions of the infra red spectrum with a thermopile implied a galvanometer of high sensitiveness and the one constructed by Nichols for these experiments had a needle and mirror weighing only 48 m. g. It was one of the most sensitive instruments of its kind that has ever been constructed.

Several very interesting facts, at that time not known, were established by these experiments. Among them were the nonselectivity of lampblack in this portion of the spectrum (out to $3.\mu$); the abruptness of the change from opacity to diathermancy of hard rubber at the edge of the visible spectrum; the increase of transmission by quartz with increasing wave length, and the fact not then generally recognized that alums in solution have no effect on the diathermancy of the water in which they are dissolved. Similarly it was shown that the solution of various substances in alcohol did not modify the transmitting power of that solvent for the rays of those portions of the infra red spectrum covered by the investigation. This research appeared in the first volume of the then newly established *Physical Review* under the title "*Transmission Spectra in the Infra Red.*" It was immediately followed by a similar study of the ultra violet spectrum which was published in the second volume of the *Review.*

The mastery of a weak-field galvanometer of high sensibility was a discipline of which the younger generation of investigators in physics have been deprived since that type of instrument

has fallen into disuse. In the daytime artificial disturbances of the earth's magnetic field made measurements well nigh impossible and when observations were to be made at night auroral disturbances would frequently drive the spot of light furiously back and forth across the scale. Indeed the faintest gleam of northern lights was a signal for postponement.

From such experiences Nichols was led to consider the possibility of a new instrument for the study of radiation; a consideration which later was to result in the development of the special tool (the famous Nichols radiometer) which he was to use in the solution of all his problems and which will ever be associated with his name.

The Berlin Period (1894-96)

In 1892 Nichols went to Colgate University as Professor of Physics, an event that was to have a profound influence upon the remainder of his life, for there he met and (in 1894) married Miss Katherine West, a daughter of one of the foremost citizens of Hamilton. In the latter year he received leave of absence and went to Berlin with his young wife for further study.

The New Radiometer

The conditions for the study of radiation were altogether favorable in Berlin and Nichols was soon on the road to the realization of his concept, often mentioned during the Ithaca days, of a glorified and refined radiometer based upon the interesting but by no means sensitive device of Crookes. This task was brought to an eminently successful conclusion with the aid of some friendly suggestions from E. Pringsheim, who had already made some studies of the Crookes instrument.[1]

The results are described in the classical paper entitled "*A Method for Energy Measurements in the Infra Red Spectrum and the Properties of the Ordinary Ray in Quartz for Waves of Great Wave length.*" [2]

The radiometer developed in the course of these experiments

[1] Pringsheim: Wiedemann's Annalen 18, p. 32 (1883)

[2] Nichols; Physical Review (1), IV, 297 (1897).

took a form from which subsequent models varied only in so far as it was found desirable to adapt the vanes to each particular case. For work in the spectrum the vanes were long and narrow. When a star image was to be measured they became disks.

In general the conditions to be fulfilled were:

(1) Lightness of the moving parts—in the Berlin instrument the mass was reduced to seven milligrams.

(2) Reduction of the torsional moment—the arm was only two millimeters in length.

(3) Use of an exceedingly light suspension fibre of fused quartz.

(4) Determination of the gas pressure (.05 *mm.*) at which the sensitiveness was a maximum.

(5) Adjustment of the vanes to the most effective distance (2.5 *mm.*) from the mica window behind which they were suspended.

Under the above conditions the radiometer gave indications equivalent to a deflection of 2,100 scale divisions when exposed to a candle at a distance of one meter.

The first investigation of importance in which the new instrument was used was an exploration of the optical properties of quartz for the longer waves of the infra red. To determine the reflecting power, that of silver was first determined as far as 9.μ, at which wave length the fluorite prism of the spectrometer became opaque. Previous measurements by Rubens had extended only to 3.μ. Shortly beyond the latter wave length silver was found to be an almost perfect reflector, the values all ranging between 99% and 100%.

The reflection from the polished face of a prism of quartz, cut perpendicular to the optic axis, was then directly compared with that from the silver mirror and the transmission measured through a plate of quartz 18μ in thickness.

Thus were discovered the minimum of reflection (0.29%) at wave length 7.μ followed by the sudden transition to metallic reflection (74%) indicative of the presence of the great absorption bands between 8.μ and 9.μ. The same set of measurements served to disclose the hitherto unsuspected anomaly in

the index refraction of quartz which had misled earlier observers as to wave lengths in these regions of the infra red spectrum.

The Residual Rays

An immediate and very important result of these experiments which were described before the Berlin Academy of Science in 1896[3] was the collaboration of Rubens and Nichols and the consequent development of the now classical method of residual rays for the exploration of the still unknown regions of the extreme infra red.

Given the existence in an optical medium of restricted regions of high reflecting power, such as had been shown to exist in the case of quartz, it became obvious that by several successive reflections from the surface of such a medium radiation could readily be obtained which would be approximately monochromatic; all other wave lengths than the one strongly reflected having disappeared by transmission through the substance. Furthermore, the location of these regions of approach to metallic reflection could be approximately estimated from the constants of the Helmholtz-Kettler formula for media the dispersion constants of which had been determined.

By three successive reflections from quartz of the radiation from a zircon disk it was found possible to isolate a bundle of rays corresponding to the band in the absorption spectrum of quartz which had been found in the course of the experiments which Nichols had just completed. To determine the wave lengths, since all material for prisms were under suspicion in these unexplored regions, recourse was had to a diffraction grating made of fine wires of gold strung side by side. This had the advantage over all ordinary gratings that the rays suffered neither reflection nor transmission by a medium other than the air. The third order spectra were readily found and the location was such as to corroborate the previous measurements. The agreement with the wave length predicted by means of the Helmholtz-Kettler formula was also as close as could be expected.

[3] Nichols: Berliner Berichte, Nov. 5, 1896; also Physical Review (1), IV, 297, 1897.

Earlier determinations by Paschen of the optical constants of fluorite afforded data by means of which it could be computed that residual rays obtained by reflection from that substance would have a wave length of about 30μ. The difficulty of confirming this prediction was really very serious. Only about one thousandth part of the initial energy remained after three reflections and to locate the diffraction spectra was a matter of the greatest delicacy.

The necessity of sending the radiation to be measured through a window into the vacuum chamber had seemed to invalidate the radiometer, since the best known transmitter of these infra red rays was fluorite itself which would be opaque to its own residual rays. A bolometer was therefore used with an armored galvanometer of high sensibility. The indications were barely perceptible, but a careful set of readings gave positive evidence of residual rays of the predicted wave length.

Here then was a promising method for the study of radiation in still more remote regions of the infra red. The previous limit of definite knowledge had been at about $9.\mu$. There was, however, indirect evidence of the existence of much longer waves obtained by comparing the absorption by various substances of the radiation from sources of high temperature and of low temperature.

Rubens and Nichols proceeded at once to test a variety of optical media, but in addition to the residual rays from quartz and fluorite they succeeded in finding only those of rock salt and of mica, which latter substance showed two bands due to silica.

Rock salt was exceedingly difficult to work with. To increase the sensitiveness of the method beyond that attainable with the bolometer recourse was had once more to the radiometer, a change made possible by the discovery that silver chloride transmits these longest waves and that a thin plate of this material could be used instead of fluorite for a radiometer window. Five reflections were needed to separate out the residual rays and these were so feeble that their wave length could not be determined with certainty by means of the diffraction grating. It was shown, however, by a study of the refractive index that the

residual rays from rock salt undoubtedly had a wave length of approximately 50μ. (Direct measurements in 1908 by Nichols and Day gave 52.3μ).[4] This was a very great extension, indeed, of the spectrum towards the longer wave lengths and a notable step beyond what Paschen and other workers in this field had thus far been able to reach.

A wave length of 50μ is one hundred times that of the green light in the middle of the spectrum. It is a distance, moreover, which lies within the realm of things directly appreciable by the unaided senses: a twentieth of a millimeter—the thickness of a sheet of paper!

In their relation to waves of these great lengths optical media might be expected to have properties quite different from those observed in the case of light waves and in the course of these researches it was noted that the metals increased in reflecting power and reached the limiting value of 100% for the residual rays of fluorite (23.7μ); also that lampblack was nearly transparent, whereas of substances transparent to the visible spectrum only three (silver chloride, sylvine and rock salt) transmitted a measurable amount of the residual rays of fluorite through a layer one millimeter in thickness.

Heat Waves and Electro-Magnetic Resonance

Having advanced the boundary of the infra red spectrum so far into the territory of electric waves the temptation to seek an electro-magnetic reaction to the residual rays was irresistible. Much time was therefore spent in the ruling of cross-hatched gratings on silvered glass, the purpose being to produce a system of resonators which might be expected to respond to rays of the wave length in question.

Righi, working with short electric waves, had obtained a maximum resonance with resonators corresponding to even multiples of quarter wave length. In making the minute rectangles of silver, therefore, the spacing of the cross-rulings was adjusted so as to give odd and even multiples of the quarter waves, respectively, on the gratings with which experiments were to be made.

[4] Nichols and Day; Physical Review (1), XXVII, p. 225.

Upon measuring the intensity of a polarized beam of the residual rays after reflection from these grids, the reflection was actually found to depend so definitely upon their dimensions, in agreement with the electro-magnetic theory of light, as to leave no doubt that the desired effect, i. e., the production of electric resonance through the action of the infra red radiation, had been successfully obtained. The authors expressed their obligation to Mr. Augustus Trowbridge, who assisted in the very difficult task of ruling the gratings.

This is a most significant experiment, second only to the demonstration of the *existence* of electric waves by Hertz. In all previous studies the attempt had been to build up a complete and convincing *parallel* between electric waves and heat waves, from which the identity of the two was to be inferred. Here, for the first time, that identity was established in a more direct and therefore a more impressive way—*by the successful substitution of heat waves for electric waves in the production of electro-magnetic effects.*

These observations did not "bridge the gap between electric waves and the infra red" in the sense of the production of electric waves shorter than the longest heat waves—or vice versa. (This was a problem to which Nichols returned many years later and which he solved.) They did, however, close the gap in another and even more important sense.

After two fruitful years in Germany Nichols returned to Colgate with his wife and a daughter born in Berlin, and resumed the teaching of physics. The results of the researches on the infra red were put into English, for publication in the *Physical Review,* and work required for the degree of D. Sc. at Cornell was completed. The examinations for the doctorate and the conferring of the degree occurred in 1897. In the meantime the radiometer, which had been found superior in delicacy to the bolometer, was being made ready for new and more trying experiments.

The Dartmouth Period (1898-1903)

Five happy and productive years in the life of E. F. Nichols began in 1898. In that year he was called from Colgate to the

Professorship in Physics at Dartmouth College and the same year saw the commencement of a particularly striking and successful investigation.

Heat from the Fixed Stars

This notable research was carried on in the heliostat room of the Yerkes Observatory, the use of which had been granted for this purpose by the director.

For these experiments the Nichols radiometer was given a slightly modified form. The vanes were circular and of a size to approximate as closely as practicable the star images of a reflecting telescope of 61 *c.m.* aperture and 233 *c.m.* focal length, the mirror of which had been figured and silvered by Ritchey especially for this work.

Thirteen years earlier C. V. Boys had exposed his radiomicrometer to radiation from the stars and so great was its delicacy that for the first time in the history of such endeavor there seemed to be a reasonable prospect of success. When the coil of the radiomicrometer was suspended in the focus of a sixteen-inch mirror a candle at a distance of 250 yards gave a deflection of 38 *m.m.* But for atmospheric absorption a considerable movement of the finely suspended thermo-element might therefore have been expected at a quarter of a mile, or possibly a discernible deflection from a candle a mile way! That no effect could be detected from any of the planets or fixed stars indicated quite clearly what the minimum sensibility of an instrument must be to give an unquestionable result. On the other hand, a doubtful motion observed when the image of Venus was thrown upon the thermo-junction of the radio-micrometer appeared to show that Boys had probably failed by a very narrow margin.

The various conditions upon which the sensitiveness of the radiometer is known to depend were therefore given especial consideration and, in spite of numerous troublesome disturbances greater than the effect itself, the first series of observations, made on August third, 1898, gave an unquestionable and positive result. In fact, it was found possible to estimate with

reasonable accuracy the ratio of the brightness of Arcturus to that of Vega.

The following record of readings made on five nights will give an idea of the smallness of the quantities to be compared; the deflections caused by the heat received by Arcturus being of the order of one millimeter, those from Vega of half a millimeter.

Arcturus and Vega Compared

Date	Deflections		Ratio
August	Vega	Arcturus	Arcturus/Vega
4	0.31 m.m.	0.65 m.m.	2.1
8	0.64 "	1.30 "	2.0
9	0.33 "	0.98 "	3.0
11	0.60 "	1.36 "	2.3
13	0.68 "	0.68 "	1.0

The sensitiveness of the apparatus was such that a deflection of one millimeter represented about *one forty-nine millionth part* of the heat received from a candle at a meter's distance.

The situation of the Yerkes Observatory is particularly favorable for the determination of the effects of atmospheric absorption since the open and nearly level country stretches out to westward for many miles. For the estimation of this correction tents were erected at distances of 2,000 and 4,500 feet, respectively, and within these were candles which could be exposed or obscured in response to signals from the heliostat room of the observatory. Mr. C. E. St. John and Mr. A. L. Colton acted as assistants.

It was found that the intervening air would transmit 52.3% of the energy from a candle at a distance of 762.4 meters and from such measurements the absorbing effect of the atmosphere between a star and an observer at the surface of the earth could be computed. From these calibrations it was also found that the Nichols radiometer was twenty-six times as sensitive as the radiomicrometer used by Boys. By so narrow a margin was success separated from failure!

In August, 1900, observations were resumed. A cœlostat

which had been constructed in May of that year for eclipse work now became available and with an increased sensitiveness of nearly fifty per cent measurements were made on Jupiter, Arcturus and Saturn. The final corrected values for relative intensities were as follows:

Vega	Arcturus	Jupiter	Saturn
0.51	1.14	2.38	0.37

The distinction between such comparisons as the above of the energy received from various stars and the indications obtained, for example by means of a photo-electric cell, is an important one. The photo-electric cell, first applied to stellar observations by Minchin in 1895, gives no direct measurement of the total heat output of a star; being even more highly selective than the retina or the photographic plate. It is photometric rather than radiometric and has therefore an entirely different function from any of the heat measuring devices. It has important applications in astronomy, but could not be used in estimating the climate of Mars or the moon, as has recently been done by Coblentz by a purely radiometric procedure.

Light-Pressure

This by-product of the electromagnetic theory was still in the class of effects mathematically established but not experimentally verified, twenty-five years after its statement by Maxwell in 1873. Nichols seems to have had it in mind, as a thing to be tried with his new radiometer, even in his Berlin days. It was then a subject of lively discussion among the younger men in the laboratory, with the general opinion rather against the practicability of an experimental demonstration unless, indeed, some superphysicist—a Paschen or a Lebedew—should make the trial. Now, at Dartmouth with the able and enthusiastic collaboration of Professor G. F. Hull the time was ripe for such an attempt.

The conditions to be met in the proposed experiments were, however, in some respects diametrically opposed to those of the radiometric process as applied to the determination of the energy of a beam of light.

In the measurement of heat, radiation reflected from the vane of the balance is lost—complete absorption is to be desired. In the measurement of light pressure radiation absorbed is only half as effective as radiation reflected. Perfect reflection is therefore the ideal condition.

In the measurement of heat, again, advantage is taken of the convection currents in the gas set up by the warmed surface of the vanes. This effect is a maximum for a certain pressure in the vacuum chamber within which the vanes hang and the best result is obtained when a certain very small distance is maintained between the vanes and window. In the measurement of light-pressure, adjustments were made with a view to reducing this so-called gas-action to a minimum and since gas-action is cumulative, whereas the response to light-pressure is immediate, a short period of vibration was evidently desirable. It was further decided that a ballistic throw should be used instead of a steady deflection.

To meet these new conditions a torsion balance with much larger vanes (12.5 *m.m.* in diameter) than those of the earlier forms of the radiometer was constructed. A considerable mass was added to the suspended system, with a magnetic needle and controlling magnet to secure a short and adjustable period of oscillation.

The vanes of microscope cover glass were silvered on one side, instead of being coated with an absorbing layer of oxide. By study of the variations in gas-action with the state of the vacuum a critical pressure (16 *m.m.* Hg.) was found at which the convection currents were scarcely appreciable. At this pressure the measurements were made.

It was the aim of this investigation to measure as accurately as possible the light-pressure resulting from exposure of the vane of the radiometer to a beam of known intensity for a given time and to compare the quantity thus obtained with that computed from the equations of Maxwell.

This comparison involved a knowledge of the moment of torsion of the radiometer which was determined by the method of vibrations. The time of exposure selected was six seconds, which was approximately a quarter of a period of the suspended system.

The intensity of the incident beam was measured, in the preliminary experiments, by means of an especially designed bolometer, but this was afterwards supplanted by a calorimetric device which was much more satisfactory. When the calorimeter was used the agreement between practice and theory was well within the experimental error.

Not only was the existence of light-pressure substantiated by this research, but the result of the Maxwellian analysis, *i. e.*, that the light-pressure is numerically equal to the energy contained in unit volume of the beam producing it, was quantitatively verified. A careful determination of the sources of error showed that the discrepancy between the observed and the computed effect was well within the probable error and that the latter was less than one per cent. It was further shown that in agreement with theory the effect is independent of the wave length of the pressure-producing ray.

A Question of Priority

The first announcement of the successful measurement of light-pressure by Nichols and Hull was made at the Denver meeting of the American Association for the Advancement of Science on the 29th of August, 1901. An abstract was printed in *Science* in October of that year and the full preliminary paper in the *Physical Review* for November. The *Astrophysical Journal* for January, 1902, also contained an abstract of the Denver paper and in the same number appeared an abstract of a paper by Lebedew of Moscow from the *Annalen der Physik* (VI, p. 433; Nov., 1901).

By a coincidence, then, the first complete reports of these two researches appeared simultaneously, *i. e.*, in the November numbers of the *Annalen* and of the *Review*. The two pieces of work were clearly wholly independent. Nichols and Hull, as stated above, had read a paper giving specific data in August and so it appeared for the moment that priority of publication, an unimportant distinction in such a case as this, should go to them.

In fact, however, Lebedew, quite without the knowledge of the American investigators, had made a report of progress at

the International Congress of Physics which met in Paris in the summer of 1900. In this he reported that his experiments up to that time had been such as to indicate the existence of an effect equal to the values computed by Maxwell and by Bartoli. No specific numerical data were given at that time. Priority in the announcement of the *general character* of the results obtained should therefore undoubtedly be accorded to Lebedew. Nichols and Hull in experiments subsequent to the publication of their paper of November, 1901, obtained experimentally the theoretical value of light pressure with an accuracy far exceeding that of Lebedew's measurements, *i. e.*, within a fraction of one per cent.[5] Lebedew's probable error would seem to have been of the order of twenty per cent.

Comets' Tails

The establishment of light pressure upon a firm experimental basis led inevitably to a revival of interest in the application of that effect to cometary theory. Arrhenius in his then recent volume on Cosmic Physics and Lebedew in a paper published in 1902 had shown that for otherwise similar spheres the ratio of radiation pressure to gravitation is inversely as the density.

In a paper in the *Astrophysical Journal* of June, 1903 (XVII, p. 352; June, 1903), Nichols and Hull considered at some length the phenomena occurring in comets on their approach to the sun. After pointing out the doubtless very complicated conditions existing in the heads and nuclei of comets they estimated that the ratio of radiation pressure to gravitation may reasonably be supposed to reach the value of 20 : 1, whereas the ratio necessary to the production of comets' tails may be taken as about 18 : 1. The production of triple tails could be explained, as they pointed out, by supposing a sifting or sorting of the fine material to take place within the comet. Radiation pressure, then, is a sufficient explanation of the behavior of comets, but there is every reason to suppose this action to be accompanied by various other effects. Small bodies

[5] Nichols and Hull; Physical Review (1), XVII, p. 26 and 91; also Astrophysical Journal, XVII, p. 315, and Proc. Am. Acad. Arts and Sciences for 1903.

when warmed on one side, for example, are strongly repelled by a sort of gas action, while a porous substance may be driven over various complicated courses by the back thrust of out-rushing particles.

To show the deflection of a stream of particles by lateral illumination, mixtures of a very light lycopodium powder and powdered emery were poured through a contraction in a glass vacuum tube, very much as in an ordinary hourglass. When a beam of light was focussed upon the stream a strong deflection of the lighter particles occurred, while the denser bits of emery passed down through the tube essentially unaffected.

As an ocular demonstration this experiment was striking, but in spite of extraordinary precautions to maintain a high vacuum the out-rush of gases from the heated surfaces of these light particles, on the sides exposed to radiation furnished a driving force several times greater than that due to light pressure itself and in the same direction.

To what extent this gas effect which Nichols and Hull picturesquely designated as "rocket action" is actually added to the light pressure in the driving away from the sun of the materials composing comets' tails is, of course, not readily to be estimated.

At Columbia (1903-1909)

After five years at Dartmouth, Nichols accepted a call to a professorship in Columbia University, which position he held until 1909. The winter of 1904-5 was spent, on leave, at the University of Cambridge, concerning which delightful sojourn Sir Joseph Larmor writes:

"I have vivid recollection of his arrival here now long ago, I think simultaneously with H. A. Bumstead, now also gone. They had the advantage of being accompanied by their families then in tender years. Although they belonged primarily to the Cavendish Laboratory and so were rather outside my own sphere, they fitted into all sides of Cambridge life at once, created the impression that they belonged to it essentially, though at the same time bringing the experience and attitude of a larger world. Especially Nichols brought with him an es-

tablished reputation in physical experiment from both America and Berlin: and his efficiency and helpfulness were aided by a modesty which was, as often, a main source of influence and authority. These pioneers were followed soon by Hull and Zeleny and many others, and the stream has not ceased. Wherever their careers took them in after life we felt that there Cambridge had a footing and that a plane of intimacy and mutual appreciation would persist."

In New York after a considerable period of readjustment to the conditions, never particularly favorable to scientific endeavor, of the city, a new attack was made upon the problem of residual rays. In Berlin the question as to the existence of the absorption bands upon which such rays depend had been left unanswered save in the case of fluorite and rock salt, and the wave length of the rays in the case of the latter substance was but roughly estimated. It was now found possible to determine the position (at 52.3μ) for rock salt with precision and to add to the list of the substances investigated ammonium chloride (51.4μ), barium carbonate (46.0μ), and strontium carbonate (43.2μ).

This addition to our knowledge was made by the use of a group of Nernst filaments as a source of light, by such refinements in the construction of the radiometer as were suggested by the previous extended experience with that instrument and particularly by the careful removal and exclusion of water vapor from the atmosphere through which the rays passed on their way from the source to the vanes of the radiometer. Aside from the isolation and determination of the wave length of these new residual rays the research brought out a relation between the wave length and the atomic weight of the bases in the salts used as reflectors, *i. e.,* that the wave length of the residual rays increases with atomic weight of the base. Dr. W. S. Day was the co-worker in these experiments.

Absence of Long Waves in the Solar Spectrum

To this period likewise belongs the very ingenious and beautiful demonstration of the complete absence in sunlight, even at the height of the Mt. Wilson Observatory, of waves of the

order of 51μ. In the summer of 1906 Nichols set up on Mt. Wilson a series of five reflecting surfaces of rock salt, and showed that a beam of sunlight after successive reflection from these produced not the slightest effect upon the vane of the radiometer. All wave lengths outside the absorption band would disappear, since the reflecting power of rock salt is about three per cent and $.03^5 = .000,000,024,3$; whereas, within the band the reflection is ninety-seven per cent, and the intensity after five reflections would be $.97^5 = .858$. So complete is obviously the separation of the residual rays from the entire remainder of the spectrum in an extreme case like this that the demonstration is conclusive.

The Presidency of Dartmouth College

In 1909 the presidency of Dartmouth College fell vacant and Nichols was sought to fill the post. That he was considered after being absent from Hanover for six years, during which time he had not concerned himself in any direct way with the affairs of the college nor shown an interest by speech or written word in pedagogy, bears witness to the profound impression upon the college community at Hanover which he had made as a man of character and ability. The same qualities which throughout his life drew others strongly to him were doubtless decisive in this instance.

That he accepted the call, although he knew that his action involved the relinquishment of research for many years to come, must be interpreted to mean that in him after all the man of science was not paramount; that the call to human service, at whatever sacrifice, would not be denied. Not all of his friends were willing to accede without question the wisdom of his decision nor the necessity for the sacrifice. One speaker at the supper held the day of his inauguration was bold enough to voice this dissent; essentially in these words:

"Men of Dartmouth: You have chosen a man to be your head who is undoubtedly capable of the highest type of service possible to a college president, but do you realize the price? You have called from his laboratory a man whose labors in the domain of Physics are immortal and through whom during his stay with you lustre and glory shone upon these college walls.

"There may be, there doubtless are, a thousand men qualified to do thoroughly well what a college president has to do. Where will you find another to measure the pressure of light or determine the heat from the fixed stars? These are prob-

ERNEST FOX NICHOLS

lems that had baffled all who came before, and he had solved them. Do you realize, moreover, that upon such work as that and upon it only, the lasting and true fame of an institution of learning rests?

"If by some cataclysm Dartmouth College, proud as it stands today, and justly proud, were to be swept from the face of the

earth, it would be utterly forgotten in a single century; save that it might be recalled that here in the opening years of the twentieth century Nichols and Hull carried on their imperishable researches. But you have called and he has come to you and laid aside his work, which none but he could do, to be the servant of Dartmouth for years to come. May you realize what a sacrifice has been made in your behalf and give to him sympathy and loyalty and support unstinted!"

And so for seven years Ernest Fox Nichols went up and down the land, as college presidents must, talking to Dartmouth alumni and friends, pleading for loyalty and fanning the flames of love for Alma Mater. And Dartmouth flourished and waxed greater. It was an important position, worthily filled, but the work had to be done, as is ever the case, at the cost of a total abeyance of all scientific productiveness. At last in 1916 it became possible to lay down this burden and to become again a simple man of science, to which good end Yale opened her hospitable doors and offered him a professorship in physics.

When America entered the World War the call for service was, of course, imperative and not to be denied. Many months were given to the investigation of the numerous schemes proposed to the government for protection against the menace of the submarine; countless conferences and consultations were held under a pressure that was inexorable. Everything—whether scientific, social or personal—had to give way to the great questions of national defense. Thus the seven years given over to the work of the Dartmouth presidency were stretched into ten before it became possible to get into harness again for research.

In 1920 came an offer of the directorship of the work in pure science at the Nela Park Laboratory in Cleveland, an opening which seemed more favorable to uninterrupted scientific activity than the professorship at Yale; and this was followed a few months later by an unlooked for call from the Massachusetts Institute of Technology.

The death of President McLaurin had left that institution without an executive head, and its friends discerned in Nichols the man preeminently fitted to deal with its problems; in par-

ticular with that of the proper relation of applied to pure science in the education of the engineer. The idealism that characterized his inaugural address, in which the human aspects of engineering were especially emphasized, attracted much favorable comment; but all hopes of his taking an active part in the realization of the vision unfolded on that occasion were thwarted by a most serious illness with which he was stricken almost immediately and which compelled many months of complete rest and relaxation. All thoughts of further administrative work were definitely abandoned and upon his resumption of a relatively active life it was to Cleveland and the scientific work of the laboratory and not to Boston that he turned.

His letter of resignation addressed to the trustees of the Institute of Technology expresses in so fine a form the noble spirit of the man that although it has already appeared in an article by his cousin, Professor Philip Fox,[6] it is reproduced below.

"A sufficient time has now elapsed since the onset of a severe illness, which followed immediately upon my inauguration, to enable my physicians to estimate consequences. They assure me certain physical limitations, some of them probably permanent, have resulted. These, they agree, make it decidedly inadvisable for the Institute or for me that I should attempt to discharge the manifold duties of president. Indeed, they hold it would be especially unwise for me to assume the grave responsibilities, to attempt to withstand the inevitable stresses and strains of office, or to take on that share in the open discussion of matters of public interest and concern inseparable from the broader activities of educational leadership.

"As my recuperation is still in progress I have contended earnestly with my doctors for a lighter judgment. I feel more than willing to take a personal risk, but they know better than I, and they stand firm in their conclusions.

"The success of the Institute is of such profound importance to our national welfare, to the advancement of science and the useful arts, that no insufficient or inadequate leadership is sufferable. Personal hopes and wishes must stand aside.

[6]Astrophysical Journal, LXI, p. 1, 1925.

"It is therefore with deep personal regret, but with the conviction that it is best for all concerned, that I tender you my resignation of the presidency of the Institute and urge you to accept it without hesitation.

"To you who have shown me such staunch and generous friendship it is pleasant to add that in the judgment of my physicians the physical disqualifications for the exigencies of educational administration are such as need not restrict my activities in the simpler, untroubled, methodical life of scientific investigation to which I was bred. It is to the research laboratory, therefore, that I ask your leave to return."

In his subsequent and what were to be his last researches, Nichols now reverted to his favorite problem of earlier years, *i. e.*, to the bridging of the gap between electro-magnetic waves and the infra red. With the collaboration of Dr. J. D. Tear two papers were published. In the first of these the boundary of the electric spectrum toward the infra red was advanced very materially by the production of Hertzian waves, which were much shorter than any previously known.

This result could be attained only by the construction of a Hertzian oscillator capable of setting up such waves and of a receiver adapted to them. The oscillator had an adjustable spark gap in kerosene oil between minute tungsten cylinders. The spark length was of the order of .01 *m.m.* The receiver was a sort of super-radiometer, the vanes of which were of tungsten or in some of the experiments of very thin mica bearing a light film of platinum. The weight of the suspended parts was reduced to about half a milligram.

This type of instrument was a result of ideas developed by Nichols nearly twenty years earlier and discussed by him in a paper before the International Electrical Congress in St. Louis in 1904. The radiometer vanes bore resonators consisting of suitable lengths of platinum wire, 1μ in diameter, mounted in front of mica shields, or of resonating rectangles of platinum film deposited on thin mica. These excessively minute elements of the receiver absorbed waves to which they were in resonance and becoming heated gave a deflecting moment to the suspended system of which they formed a part. The deflection

was proportional to the incident energy. Such a radiometer, in which the heating effect is due to resonance is selective and reacts only to waves with which it is in tune.

The gap in the spectrum at the time these measurements were begun was from 0.4 *m.m.* (the longest heat wave recorded by Rubens and von Bayer in 1915) to 7.0 *m.m.,* the shortest electric wave (Mobius, 1920).

Ignoring the overtone of wave length 0.8 *m.m.* the boundary was thus extended by over two octaves at the time of the completion of the paper described above. In reality, since there is no good reason for rejecting the overtone the extension towards the red was more than three octaves.

A few months later, in time for the spring meeting of the National Academy in 1924, the authors were in position to announce the complete filling of the gap.[7]

Using an oscillating doublet in which the tungsten cylinders were only a tenth of a millimeter long and a tenth of a millimeter in diameter, fundamental waves of .9 *m.m.* were obtained. Adjustment of the conditions of vibration made it possible to suppress the fundamental and enhance a chosen overtone. In this way electric waves *down to .22 m.m.* were produced.

It only remained to establish, independently of other observers, an overlapping boundary of the infra red spectrum to which end advantage was taken of the existence, already known, of very long waves in the radiation from the mercury arc. Filtration through eight millimeters of fused quartz and from sheets of black paper removed the shorter rays from this source and a beam was thus isolated in which *those of .42 m.m. predominated.*

Wave lengths of both electric and infra red rays were determined with a Fabry and Perot interferometer and an especially designed echelon.

The gap between the two spectra was thus definitely and effectively closed, with an overlap of nearly two millimeters.

The presentation of this final paper took place in the beautiful new hall of the Academy of Sciences, which had been opened and dedicated with imposing ceremonies the day before and

[7]Nichols and Tear; Astrophysical Journal, Jan., 1925.

was in use for scientific deliberations for the first time that morning. The assemblage was one of unusual eminence, the occasion an impressive one, with no suggestion about it of impending tragedy. The speaker with his story half told was in the midst of a lucid statement, quite in the happy manner usual to his presentations, when his heart stopped. His sentence remained unfinished and in an instant in the midst, as it were, of friends and colleagues, his earthly career had its sudden ending. The spirit of Ernest Fox Nichols had passed on into the other world. What more fitting departure could a man of science desire?

The Ending of the Day's Work

Some of the most significant comments of the daily and the technical press were gathered into a leaflet and printed under the above title. They are so sympathetic in tone and they voice so well the feelings of the scientific world that it has seemed fitting to reproduce and thus preserve them as an addendum to the present biographical notice.

From the *New York Times,* April 30

(Abridged)

Washington, April 29.—Dr. Ernest Fox Nichols, one of the leading members of the National Academy of Sciences, dropped dead shortly after noon today while addressing the great audience of scientists at the three-day meeting of the National Academy of Sciences and National Research Council.

Just as he was concluding an important address in the new $5,000,000 endowment building of the National Academy which President Coolidge assisted in dedicating yesterday, Dr. Nichols collapsed, but as he did not fall, the academicians present did not at first grasp the full import of the tragedy that had just occurred before them.

Dr. Nichols stood on a semi-circular marble platform of Grecian design from which President Coolidge and eminent scientists have spoken during the past two days, and near the end of the address uttered the worlds: "I must—" at the be-

ginning of a sentence which he never completed. Then he moved back toward the center of the platform, leaned over toward the right, until the weight of his body was resting against the marble stand.

Some present thought he was merely resting, as it was known his heart was not in good condition. Others, in view of the fact that Dr. Nichols' talk related to new radiometric electric wave receivers, thought he was leaning over to test some instrument. However, when he failed to move, scientists rushed to the platform, and observed at a glance that he was dying.

C. M. Jackson, head of the Medical Division of the National Research Council, was called to the platform. An ambulance was summoned from the Emergency Hospital, but had some difficulty in finding the building, which was only opened for the first time yesterday. Major General George O. Squier, former head of the Army Signal Corps, summoned an Army ambulance. Before either ambulance arrived the eminent scientist had expired.

The scientists formed a double line through which the body was borne from the building. The flag on the Academy was half-masted.

From the *Springfield* (Mass.) *Republican*

(Abridged)

News of the sudden death of Dr. Ernest Fox Nichols will come as a shock to a host of men who knew him as college teacher and president. As President of Dartmouth seven years and of Massachusetts Institute of Technology eight months, and as professor at Colgate, Dartmouth, Columbia and Yale, he has been a guide and counselor to nearly a generation of students.

Dr. Nichols' success as an educational administrator was testified to in the most cordial terms by the trustees of Dartmouth in their letter accepting his resignation, "with the greatest reluctance," in 1915. He had, they said, brought to bear upon the processes of readjustment at the college "a high order

of administrative ability, enriched with a large tolerance, an exhaustless patience, a noble dignity and generosity"; "by unremitting labor," he had "accomplished a monumental task"; he had brought to his task "trained powers of analysis, coupled with the loftiest ideals of scholarship," and it had been their "hope that Dartmouth College might long continue to enjoy" his leadership. That his reputation in the educational field was excellent is further evidenced by his choice later as President of "Tech," a position which ill health compelled him to resign after a brief service.

. But notwithstanding Dr. Nichols' constructive interest in educational problems and his recognized success in meeting them, it was as a scientist that his greatest work was done and his greatest reputation acquired. In resigning from the presidency of Dartmouth to take the chair of physics at Yale, he recalled that he had accepted it reluctantly, his work as teacher and student of physics having gratified his "every ambition"; he also felt he had "already done" his best work for the college. In a statement at the time he further said that he felt "like one who has been loaned by one profession to the other" and that the "time for payment" had arrived.

As a physicist his rank is with the best that America has produced. His experiments, discoveries and inventions in the branch of the science dealing with heat and light rays and with optics gave him an international reputation. His taking off while still in possession of a keen mind and eager enthusiasm is a great loss to human knowledge.

Editorial, *Electrical World,* May 10

A tragedy of great impressiveness occurred at the new and handsome hall of the National Academy of Sciences in Washington on the morning of April 29th. It was the first session of the Academy in the new building, following the dedicatory exercises of the preceding day. Several interesting papers were read according to schedule, and about 11 a. m., Dr. E. F. Nichols, the widely known physicist, went on the platform to read a paper on "Joining the Infra-Red and Electric Wave Spectra." He had been for a long time in poor health, but

did not show any signs of weakness as he went up to speak to the academicians in their fine central auditorium. With clear voice and fine delivery he presented his subject, which was of great interest to all electrical men. His paper was illustrated by slides, and he spoke freely without notes.

His first slide showed a long vertically drawn column spectrum of electromagnetic waves. On the top of the column were the shortest known X-ray waves, of length 2 tenth-centimeters (2×10^{-10} cm.). At the bottom of the column was the approximately longest radio wave of 3 centimeter-sixes (3×10^6 cm.), or 30 km., corresponding to a frequency of 10 kilocycles per second. This range of 1.5×10^{16} in ratio, or some 54 octaves, showed only about one octave of visible spectrum, near the fourth-centimeter point (1 micron). He showed that in all this long pathway of 54 octaves there had been left one gap of unexplored frequencies, having wave lengths a few centimeters long, where the infra-red joined on to the Hertzian short electromagnetic waves. He described how his work, still in progress, with a collaborator, J. D. Tear, closed that gap and made the whole series a continuously explored spectrum.

Very clearly and beautifully he described the stereopticon pictures, showing the methods and apparatus he used, with little Hertzian cylinders and with radiometers, to produce, detect and measure these little electromagnetic waves. He showed a most interesting picture of an echelon grating, composed of successive brass slabs, forming a doll's stairway, backed by an inclined glass sheet. He remarked as he pointed these out on the screen, "I think this will interest our president, Dr. Michelson," who sat on the platform opposite to him. He turned to the next slide, which showed two curves, one according to theory and the other to observation, concerning the behavior of this echelon. Without change in voice or bearing and without premonitory symptom, he said as the slide changed, "I shall—" and sat on the rear bench of the platform, laying down his head on it, with the pointer still in his hand. So quietly was this said that even the president, on the platform near him, supposed that he had merely bent over for a few moments. For fifteen seconds the hall was perfectly still.

Then the president went over and touched him gently. A physician was called for. When the physician went up Dr. Nichols was pulseless and showed no signs of life. The session immediately adjourned.

There was something very fine in this tragic close of an active career. A great scientist went to his rest, without suffering and suddenly, while narrating his victory over the unknown to his fellow academicians, with his wife and friends by his side. The triumph exceeded the calamity of his death. He closed the gap as he departed.

From *Science*, May 9, 1924

Surrounded by friends and associates and addressing, as one of the leaders in his own field of science, a distinguished audience of fellow scientists, Ernest Fox Nichols died suddenly April 29, in the auditorium of the building of the National Academy of Sciences at Washington.

Were it possible to be unmindful of the added shock and sorrow to his family which such sudden death brings, his friends could have wished for him no more fitting ending of his life, devoted as it was to the advancement of scientific knowledge, than to die in full mental vigor, in physical health as it seemed, and to the very last instant taking the part of a leader in his profession.

Ernest Nichols was born fifty-four years ago and received his collegiate training in his native State, Kansas. As a graduate student in physics he began his career at Cornell University from which institution he received the doctorate of science in 1897 after having spent two years at Berlin, where he completed his first important piece of experimental work. Unlike the majority of foreign students in Berlin in those days, Nichols worked on a problem of his own devising. He appeared older and more experienced than most of his fellow students in the laboratory, though he was not yet twenty-five years old, and his assiduity and his patience in overcoming great experimental difficulties was amply rewarded by his producing a very fine piece of work. This first important research of his was the study of the optical properties of quartz in the infra-red region

of the spectrum and the results which he obtained led directly to the perfection of the so-called method of residual rays which has been used with conspicuous success by Rubens and his fellow workers in investigating the extensive infra-red spectrum. Before he left Berlin at the end of his second year, Nichols had published important papers in collaboration with Rubens and he was regarded both in Europe and America as an experimental physicist of extraordinary ability. In the course of the next ten years he held successively the positions of professor of physics at Colgate, Dartmouth and Columbia, and during this period his research work was largely directed towards the experimental verification of certain predictions of the dominant electro-magnetic theory of light. One of these predictions, that a beam of light should exert a minute pressure on an object in its path, had been looked for without success until Nichols, in collaboration with Hull, in America, and Lebedew in Russia, independently discovered and measured this minute effect and found it to be in accord with the theory.

In 1909 he gave up for a period of seven years his chosen field of work to become President of Dartmouth College. Throughout this period, embracing as it did the best years of his professional life, he cherished the hope that he might return to the life of productive scholarship which he had had to abandon in assuming heavy administrative duties. In 1916 he went to Yale as professor of physics, but his hopes of leisure for research in pure science were not to be fulfilled, as the approaching entry of America into the World War made it necessary to organize the scientists of the country for research and invention along lines having immediate practical value in war. Dr. Nichols was among the first to offer his services to the government through the National Research Council, which he had helped to organize, and he was an active member of the group engaged in the study of antisubmarine defense in the early part of the war and was connected with the department of Naval Ordnance during the entire period of America's participation in the war.

In 1920 he became the director of research in pure science in the laboratory of the National Lamp Works in Cleveland,

a position which, but for a short period of time, he occupied until the time of his death. In 1921 he was inaugurated President of the Massachusetts Institute of Technology, but owing to serious ill health was unable to continue in office for more than a few months.

Of the thirty years between the publication of his first important paper and his death, about one-third was devoted to purely administrative work and this period was that during which discoveries of the most far-reaching importance to physics were being made. When Nichols returned to experimental investigation, he felt that he had almost to learn his own subject over again, and he told many of his friends that it seemed to him that he should never regain a firm grasp of it. He did this, however, in spite of delicate health, and in the end was contributing regularly to the physical journals and reading papers before the American Physical Society and the National Academy of Sciences. His modesty with regard to his own place in American science was so great that one wishes he might have known what was to happen at his death: that the most distinguished gathering of his fellow scientists of America were to stand uncovered, bowed and sorrowful, at the tragic loss of an honored colleague as his dead body was borne through their ranks.

AUGUSTUS TROWBRIDGE.

PRINCETON UNIVERSITY.

From *Light*, June, 1924

The tragically sudden death of Doctor Ernest Fox Nichols while delivering an address before the National Academy of Sciences in Washington, brought profound sorrow to a great company of his friends and followers in all walks of life and in every part of the country. The entire body of his National co-workers join in expressing to Mrs. Nichols and Miss Nichols our sincere sympathy in the sorrow which has fallen upon their hearts and home.

Among the leaders in the Nela family he held a place of peculiar honor and affection. In the world of science he was a recognized authority, taking rank among the greatest. As a

citizen, he stood for those high ideals and values which constitute the great constructive energies in our national history.

Doctor Nichols was great as a scientist but greater as a man. His scientific knowledge was warmed by a beautiful human sympathy. In his nature there occurred the unusual combination of a powerful and penetrating intellect and a rare and delicate affection for folks. His keen sense of humor expressed itself in kindly judgments of men and events, invariably marked by tolerance and charity. His eyes were ever toward the light and in spite of physical weakness he faced the future with undaunted courage and hope. Conscious of the infinite mystery which enwraps the world, he held firmly to those eternal verities which, when knowledge fails, reveal themselves to faith. Of him it can be said in simple truth, "He did justly and walked humbly before his God."

The academic distinctions and achievements of Doctor Nichols were many and varied. Born in Leavenworth, Kansas, June 1st, 1869, he took his B. Sc. degree from the Kansas Agricultural College when he was nineteen. Later he pursued his studies in physics, receiving various degrees in Cornell, Berlin and Cambridge Universities. At the early age of twenty-three, he was appointed professor of physics at Colgate. From 1898 to 1903, he was on the teaching staff at Dartmouth. In 1903 he went to Columbia as professor of experimental physics. From 1909 to 1916 he was President of Dartmouth College. After a four-years' term as professor of physics at Yale, in 1920, he became director of the pure science department of the Nela Research Laboratories, Cleveland.

In 1921 Doctor Nichols was elected President of Massachusetts Institute of Technology. His inauguration brought together an unique assemblage representing the best in our academic, industrial and political life. On that occasion Governor Channing Cox of Massachusetts, a former student of Doctor Nichols, paid his old friend and teacher a remarkable tribute in a speech of extraordinary charm and feeling.

In his inaugural address, Doctor Nichols developed ideals for the engineering and scientific professions which caught the instant attention of the country. His thesis was that the engi-

neer who dominates the present age must give the same attention to the problems of human relations and human engineering that he does to the technical problems of his profession.

A few days later Doctor Nichols was stricken with serious illness. Finding it impossible to take up the duties of administration, he resigned and set himself to the task of recovering his health. Although far from complete recovery, after a year of rest he was welcomed back to the Nela Research Laboratories, where, little by little, he resumed the work of original research in which he always delighted. And it was during this period that, with his colleague, Doctor J. D. Tear, Doctor Nichols was able to bridge the gap between radio waves and light waves, thus helping towards a final answer to the question "What is Light?" which for several hundred years has furnished men of science with a theme for investigation and discussion.

Was it prophetic of the last great Adventure, so near at hand, that our friend should have given the closing days of his distinguished career to solving the question "What is Light?" As with reverent mind he sought his way amid the mysteries and wonders of physical light, who can doubt that this gallant soldier of the Truth was being fitted to behold that "Light which is the life of men?"

CHARLES A. EATON.

NELA PARK, CLEVELAND.

Nothing is here for tears, nothing to wail
Or knock the breast, no weakness, no contempt,
Dispraise or blame, nothing but well and fair
And what may quiet us in a death so noble.
—*Samson Agonistes.*

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BIOGRAPHICAL MEMOIR

OF

GRANVILLE STANLEY HALL
1846-1924

BY

EDWARD L. THORNDIKE

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It is undesirable to follow the usual custom in respect of the nature and extent of this memoir. Stanley Hall has written his own life*; the American Psychological Association in the memorial meeting and publication has provided an extensive review and evaluation of his characteristics as investigator, scholar, and teacher.† It would be idle to issue an inferior copy of these. In these circumstances it is best to record here only the essential facts of his life and work and writings.

Life

Granville Stanley Hall was born of old New England Puritan stock in Ashfield, Massachusetts, February 1, 1846. He died April 24, 1924. He married Cornelia Fisher in September, 1879. His second marriage was to Florence E. Smith in July, 1899. He had two children, one of whom, Julia Fisher Hall, born May 30, 1882, died in childhood. The other, born February 7, 1881, is Dr. Robert Granville Hall, a physician.

His childhood was spent in Worthington and Ashfield with such educational advantages as parental devotion and the local school and academy could provide. He was interested in animals and bodily skill as most boys are, and in reading as most gifted boys are. Writing, oratory and music were special interests.

At sixteen he taught school, many of his pupils being older than he. At seventeen he went for a year to Williston Seminary. The four years from the fall of '63 to the spring of '67 were spent at Williams College, where he read omnivorously in literature and philosophy, and developed a keen desire to study further. At his graduation in 1867 he was chosen

* Life and Confessions of a Psychologist, 1924, pp. IX-623.

† Psychological Review, vol. 32, no. 2.

as Class Poet, and elected to Phi Beta Kappa. The next year he was a student at Union Theological Seminary, and for the three years following in Germany at Bonn, then at Berlin. Returning to New York in 1871 he re-entered Union Theological Seminary and received the degree of Bachelor of Divinity. For a year and a half he acted as tutor in a private family in New York.

In the fall of 1872 he went to Antioch College as professor of English Literature, and later taught modern languages and philosophy. Wundt's 'Grundzige der Physiologischen Psychologie' appeared in 1874, and Hall was probably one of the first men in America to read and appreciate it. For in the spring of 1875 he had decided to go to Germany again and study with Wundt the new science of Experimental Psychology at the Leipzig laboratory. He was induced to remain another year at Antioch and circumstances led him to delay his European studies for two years more * while he taught English at Harvard, and completed work for which he was awarded the degree of Doctor of Philosophy, in June, 1878. His thesis was on 'The Muscular Perception of Space.'

From July, 1878, to September, 1880, Hall studied at Berlin and Leipzig. From the fall of September, 1880, to the fall of 1882 he lived near Boston, studied, wrote, and lectured as opportunity offered. In the second year he gave a short course of lectures at Johns Hopkins, and was offered a regular post on the staff to organize a laboratory and teach psychology. He entered upon this work in the fall of 1882. In 1884 he was made professor of Psychology and Pedagogics. Dewey, Cattell, Jastrow, Sanford and Burnham were among his students. He resigned this position in June, 1888, to become President of what was to be Clark University.

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* Two years according to Wilson (Life, p. 63-64), though Hall himself (Life and Confessions, p. 204) seems to consider that he spent one year at Harvard and then three years in Germany, instead of two years at Harvard and two in Germany, from September, 1876, to September, 1880. The time and place of the award of the Ph. D. seem conclusive evidence that Wilson is correct.

Part of his first year as president was spent abroad in conferences with experts in higher education. From April, 1889, he was at Worcester, busy with the organization of the University. Clark University opened in October, 1889, with high hopes. Mr. Jonas G. Clark, the founder, stated his intent in these words:

"When we first entered upon our work it was with a well-defined plan and purpose, in which plan and purpose we have steadily persevered, turning neither to the right nor to the left. We have wrought upon no vague conceptions nor suffered ourselves to be borne upon the fluctuating and unstable current of public opinion or public suggestions. We started upon our career with the determinate view of giving to the public all the benefits and advantages of a university, comprehending full well what that implies, and feeling the full force of the general understanding, that a university must, to a large degree, be a creation of time and experience. We have, however, boldly assumed as the foundation of our institution the principles, the tests, and the responsibilities of universities as they are everywhere recognized—but without making any claim for the prestige or flavor which age imparts to all things. It has therefore been our purpose to lay our foundation broad and strong and deep. In this we must necessarily lack the simple element of years. We have what we believe to be more valuable—the vast storehouse of knowledge and learning which has been accumulating for the centuries that have gone before us, availing ourselves of the privilege of drawing from this source, open to all alike. We propose to go on to further and higher achievements. We propose to put into the hands of those who are members of the University, engaged in its several departments, every facility which money can command—to the extent of our ability—in the way of apparatus and appliances that can in any way promote our object in this direction. To our present departments we propose to add others from time to time, as our means shall warrant and the exigencies of the University shall seem to demand, always taking those first whose domain lies nearest

to those already established, until the full scope and purpose of the University shall have been accomplished.

"These benefits and advantages thus briefly outlined, we propose placing at the service of those who from time to time seek, in good faith and honesty of purpose, to pursue the study of science in its purity, and to engage in scientific research and investigation—to such they are offered as far as possible free from all trammels and hindrances, without any religious, political, or social tests. All that will be required of any applicant will be evidence, disclosed by examinations or otherwise, that his attainments are such as to qualify him for the position that he seeks." (G. Stanley Hall, by L. N. Wilson, 1914, p. 77 f.)

Hall chose an extraordinarily gifted group of men for the faculty, but the financial support expected from the founder was not provided * and there were many resignations in 1892. The years from 1890 to 1900 were full of anxiety and of the hope deferred that maketh the heart sick. After the third year Hall not only managed the institution but taught and supervised research until his resignation in 1920 at the age of seventy-four.

He writes, "During the first three years all my time had been absorbed with Mr. Clark and in the work of the development of administration, but now the withdrawal of Mr. Clark, the hegira to Chicago, and the peace and harmony that followed left me free to take up my own work as professor, which I did with enthusiasm, although as I had delegated the experimental laboratory work to my colleague, Dr. Sanford, who was developing it so successfully, my chief activity was henceforth in other fields of psychology. . . . I had acquired a distaste for administrative work and realized that there was now very little for a president to do and that I could earn my salary only as a professor." (Life and Confessions, p. 303 f.)

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* Mr. Clark gave a fund of $600,000 and made contributions of $50,000, $26,000, $12,000 in three successive years, but thereafter nothing, except by his will at his death in 1900. His estate was much less than had been expected.

He spent a large part of every summer in outside lecturing, for the most part at university and other summer schools. He estimated near the end of his life, that he had given in all over twenty-five hundred such outside lectures, or about eighty a year. He was tireless in his devotion to students. Each day according to his biographer he spent "from three to four hours in conference with individual students." His Seminary met weekly in the evening from 7:00 to 11:00.

Hall gave his life to activities which he thought would advance psychology and educational reform with extraordinary energy and singleness of heart. He read omnivorously. William James said of him:

"I never hear Hall speak in a small group or before a public audience but I marvel at his wonderful facility in extracting interesting facts from all sorts of out of the way places. He digs data from reports and blue books that simply astonish one. I wonder how he ever finds time to read so much as he does—but that is Hall." (Wilson, p. 95.) He wrote far more than any other psychologist, as his bibliography abundantly shows. He assumed the financial and editorial responsibility for the first psychological journal in America, which he founded in 1889. Four years later he did the same for the *Pedagogical Seminary,* a quarterly to encourage scholarly and scientific work in education. In 1904 he founded the *Journal of Religious Psychology,* and in 1917 the *Journal of Applied Psychology.* He led in the foundation of the American Psychological Association, in the meetings of which he was for many years active. He was always loyal to psychology and to psychologists; and the savings made possible by a life devoid of ostentation or self-indulgence he bequeathed as a Foundation for research in genetic psychology.

Work

Hall was, from his student days to his death, interested in philosophy, psychology, education and religion in every one of their aspects which did not involve detailed experimentation, intricate quantitative treatment of results, or rigor and subtlety

of analysis.* There was, however, an order of emphasis, the years from '80 to '90 being devoted to problems of general psychology and education, those from 1890 to 1905 being especially devoted to the concrete details of human life, particularly the life of children and adolescents, and those from 1905 on being more devoted to wide-reaching problems of man's emotional, ethical and religious life.

A consensus of present opinion would choose as his most important contributions to psychology, first, his advocacy and illustration and support of the doctrine that the mind, like the body, can be fully understood only when its development in the individual and its history in the animal kingdom are understood; and second, his pioneer work in investigating the concrete details of actual human behavior toward anything and everything, dogs, cats, dolls, sandpiles, thunder, lightning, trees, clouds, or what not. He had a large share in teaching psychology to be genetic and to study all of human life.

The healthy truth of the first contribution was blurred by his insistence upon an extreme form of the theory that the growth of the mind of the individual recapitulates the mental history of its ancestors, and by his assumption that acquired mental characteristics are inherited. But those who oppose Hall's detailed conclusions about ontogeny and phylogeny could gladly acclaim the beneficent influence of his general point of view. The second contribution was marred by an apparently extravagant and illegitimate use of the questionnaire method of collecting facts, which, indeed, in the hands of some of Hall's followers, seemed almost a travesty of science. The general effort to learn more of man by studying his actual detailed responses has been very fruitful. Hall himself seems to have thought that his later contributions to the psychology of conduct and emotion were more important than the contributions to genetic psychology and child study of his middle period, and perhaps he knew best.

* Hall did much patient experimental work during his second study period in Germany and while he was professor of psychology at Johns Hopkins; and supported it always. But it was done under the stimulation of circumstances rather than the impulsion of his own nature.

His influence upon education, from the first study of "The Contents of Children's Minds" in 1883 to his paper on "Movie Pedagogy" in 1921, was an argument and plea for adapting educational procedures to the natures and needs of children. This too suffered from exaggerations and excrescences, and some of the educational psychology derived from it lends itself readily to caricature, as a sort of nineteenth century Rousseauism. Yet, by a very large majority, the leaders in the best present theory and practice in education, philanthropy, and religion will gladly acknowledge the indebtedness of their fields of work to the so-called "child-study" movement and to Stanley Hall as its leader.

It is the opinion of the writer that Hall was essentially a literary man rather than a man of science, and artistic rather than matter-of-fact. He had the passion to be interesting and the passion to convince. He was not content with an intellectual victory over facts of nature, but must have an interesting, not to say exciting, result. This result he felt as a message which he must deliver to the world as an audience. It is true that he used his extraordinary intellect and energy to discover facts and defend the hypotheses about facts in which he was so fertile. But he was not content with discovery alone, nor with the approval of a small body of experts whose verdict would decide whether his work was without flaw. Nor did he have the omnivorous appetite for truth-getting all along its course, from the details of improving apparatus or observational technique at the beginning to the mathematical treatment of comparisons and relations at the end, which is characteristic of so many modern workers in science. The truth he sought was preferably important, bearing directly upon great issues, pregnant with possibilities of evolution and revolution.

To this literary quality, we may perhaps attribute the fact that his theories rather than his discoveries are quoted, and the further fact that so many of his colleagues in psychology were confident that, in this and that particular, they were right and that he was wrong, though they would most heartily admit that his was a far abler mind than theirs. Some of them indeed thought that his great abilities were too often used in the in-

terest of undeserving doctrines, and were amazed and irritated by this.

In estimating Hall's work as a psychologist we are not left to such an evaluation as I have given. The American Psychological Association held a special session in memory of Hall in December, 1924, commissioning one of his colleagues at Clark (W. H. Burnham) to speak of his personal qualities, and one of his former students (E. D. Starbuck) to speak of his work as thinker, writer, and teacher. Dr. Starbuck chose to present a summary of the opinions of the members of the Association, one hundred and sixty-five of whom responded to a questionnaire concerning "what Hall has meant to you personally, in a psychological way, what he has contributed or failed to contribute to the subject, and the relative merits of his various studies." This summary may be given here in Dr. Starbuck's own words as an estimate of Hall's work. He said:

"When asked to have a part in this program I was reluctant to undergo the delicious ordeal. It did not seem to me humanly possible for any one properly to evaluate Hall as a psychologist, for surely he is the most intricate, dominant, involved and self-contradictory personality that has come upon the psychological horizon. I finally consented only after hitting upon the idea that you should all be asked to participate by confessing what Hall has meant to you personally in a psychological way, what he has contributed or failed to contribute to the subject and the relative merits of his various studies. I would be your scribe and secretary, I promised, and give back to you as faithful a composite picture as possible. You have done your part delightfully. One hundred and sixty-five of the members responded, a good many with such care that the papers, by consent of the writers, must be turned over to some one who is to write a life of our colleague whom we honor.

"That I should be a cataloguer of opinions and that I should even place in your hands a digest of some of your judgments seems not out of tune with the proprieties of the occasion. It is the way of going at the job that Hall himself would have liked best. A member of our craft who is now occupying an

administrative position, though not a teacher of psychology, writes: 'I am sure the report you are preparing will bring much pleasure and satisfaction to Hall himself.' No matter what one's eschatology, there is here a safe criterion of good taste; if our friend were meeting with us in real presence, what would he most enjoy? I am sure he would find pleasure in the graceful words of appreciation expressed by my colleagues on this program. His spirit would glow also in feeling out the sentiments of appreciation that stir our hearts but can find no words with which to become articulate. I think he might like best of all that we move right on and take account of stock while we ask in candor and integrity of thought what his real successes and failures have been after more than half a century of honest striving. That was the Hall way. He kept on psychologizing to the very end. He was not only a sensitive soul but a rugged and sportsmanlike spirit as well. When senescence threatened to slow down at last that perennial youthfulness that skated at sixty and laughed and worked through the seventies, his quickened thought grew sharper, attacked his pursuer as a problem and made out of it a dissertation that opened up what one of our contemporary biologists designates as a whole new branch of biological science. When he saw the Fates edging in to draw a curtain across his career that would land him in defeat or dark mystery, so far from closing his eyes and turning away, he plied these sinister presences with a thousand questions about the secrets they were hiding. Not being able to forsake his psychological sense for sentiment, as if he were, for example, a professor of physics, he at least wrung from them enough of prognostication about immortality that his fellows have judged it a contribution not without merit. So that on this occasion when every word spoken might well be the note of a majestic Requiem or a Dead March or an Heroic Symphony it is not inappropriate to glance at a table of rankings and ratings. Hall would like it—at least he would in his graceful manner make merry over it with the remark, perchance, 'This is indeed Inferno that I should be plagued even now with a statistical table.'

You might check one of the first two columns and also one of the last five. (Inquiry sent to all members of the American Psychological Association.) 160 members responded; 123 sufficiently complete to tabulate. Clark University men and other than Clark men tabulated separately.

Problems	Rating		The Pioneer		A Pioneer		Valuable		Useful		Slight		Negligible		Negative	
	Non-Clark	Clark	N-C.	C.	N-C.	C.	N-C.	C	N-C.	C.	N-C.	C.	N-C.	C.	N-C.	C.
Studies of Childhood	10.99	11.83	20	11	47	18	26	10	12	2			1			
Adolescence	10.85	11.44	33	20	35	7	33	12	11	2					1	
Human Genetics	10.4	11.36	11	13	48	14	28	9	9	2	5	1			1	
Senescence	10	10.95	21	12	27	11	30	10	15	3	4	2				
Educational Problems	9.4	11	4	3	31	16	36	16	14	1	2		2		1	
Sex	8.8	9.8	7	2	31	17	29	11	15	6	5	1	2		4	1
Play and Recreation	8.75	10	4	1	19	15	34	13	23	5	2	1				
Morale	6.77	12		3	12	9	15	8	25	11	8	1	1		1	
Fear	8.2	10.82	5	3	15	12	26	10	14	5	7	2		1	1	1
Anger	8.1	9.2	4	3	16	12	20	8	17	6	7	2			1	1
Character Education	8.11	8.07		1	13	7	25	10	25	7	1	1		1		1
Interpretation of Thought Movements	7.11	8.03		1	11	6	15	11	20	10	6	1	2	1		
Methods of Research	6.84	7.33	2	1	22	14	19	4	18	9	13	5	5	3	4	1
Jesus the Christ	6.82	9.81	5	4	12	11	19	12	18	4	5	2	7		4	
Experimental Psychology	6.55	6.1	3	2	25	13	10	5	15	8	11	6	12	3	4	
Psychology of Religion	6.42	9.95	1		24	19	22	11	18	7	9	1	5		1	
History of Psychology	6.39	7.86			6	6	21	13	18	8	7	1	5	2		
Race Psychology	6.2	7.31			15	11	9	4	17	10	12	5	4	2	2	
Psychology of War	5.46	7.6		1	6	7	13	7	23	10	11	4	3		1	
Mental Heredity	5	6.31			7	10	11	4	13	8	12	4	6	4	4	2
Immortality	3.5	6.6		1	6	6	4	6	10	4	10	6	7	3	6	
Eugenics	5.3	5.3		1	9	4	9	5	19	4	13	8	6	4	1	
Animal Genetics	3	6.5		1	2	8	3	5	12	8	13	4	13	4		1

Ranking of Hall	Number	First	First Ten	Second Ten	Upper Quartile	Second Quartile	Average	Other
Non-Clark	84	4	59	8	10			1
Clark Men	29	3	24	1	1			

"It would in the first place touch off the vanity of any one who was blessed at all with a measure of pride to be rated so uniformly high among the world's psychologists. All except 21 out of 120 place Hall at least among the first ten. Seven give him highest rank. Six others rate him as either second, *i. e.,* next to James, or among the first four or five. There can be no doubt that Hall's name is written near the peak of the pinnacle of fame within his field. These eight sentences from as many different members of this organization are characteristic: 'I think Hall stands fairly supreme among all the great leaders in psychology'; 'I always felt that G. Stanley Hall lent dignity and grace to the entire field of psychology'; 'He is unquestionably one of our foremost psychologists'; 'I have always grouped Hall with James and —— for vigor of mind and stimulating influence'; 'In many ways he made present American psychology'; 'He has thrown more problems in my way than has any other American psychologist'; 'Hall seems the most conspicuous figure in American psychology'; 'Hall seems to me to have been the man most fertile in suggestions, the most original in his ideas, and the greatest teacher America has seen.' Not one of these representative sentiments just quoted is from a Clark man and none of them are from persons who ranked him first.

"There is, as would be expected, a wide range of estimates. Hall, like any other person who has lived dynamically and dramatically, has run both limits in the scale of excellence according to his friends, all the way from near sainthood to something very inglorious. One person remarks with some fervor, 'Among contemporary psychologists I would place G. S. H. as of a rank below zero.' Another, who has not transfixed him with a point and stuck him on a scale, would doubtless have placed him far below par. He says, 'Hall was an unaccountable genius. I never believed him normal.' We then take wide sweeps on up to the norm of judgment, which is among the first ten, and on to those who give him unstinted admiration. One remarks, 'Though there are many eminent psychologists and educators among our contemporaries, there is none who can claim to equal his achievement.' Another says,

'Dr. Hall started more lines of new thought and set more persons to thinking psychologically than any other person in all history. Some others have been more methodical, have developed more refined technique, and have been better text-book makers. But no other one has had such vision. The analysts, the measurers, the experimentalists, the psychoanalysts, the applied psychologists, all must pay tribute to him for having opened up new vistas in their respective fields. He is the one really original thinker in psychology in all history. James comes next. Some others I could name who are called great are mere technicians. He is the Edison of psychology.'

"Although these estimates seem rather enthusiastic, it is an interesting fact that those of Clark and every other school who rate Hall among the first ten, no matter how terrible their criticisms, tend to betray a higher estimate than their rating would indicate. One of them, this time of the Clark vintage, a chronic and successful rater, says, 'My chief benefit from Hall's teaching and writing has come in the form of inspiration. There was a time when I took for granted that his scientific views were sound. For years I have been able, I think, to reap considerable benefit from his books and articles without believing much that he wrote.' Then after some trenchant criticism, this person continues: 'But when everything is considered I doubt whether more than three or four psychologists in the last fifty years have done more for their subject than Hall.' Another who rates him after the mode says, 'He has been the cause for more writing and research than any other three men in the field.'

"As an illustration of how, in spite of the terrors of the antipathies against Hall and his school, fanned often into a flame, perhaps, by jealousies and rivalries, the majesty of his personality and of his idealism commands recognition, I can do no better than quote from the letter of one of our most respected technologists: 'I was a student at —— when 'Adolescence' appeared. I read many chapters, my impression growing all the while that the writer was very fertile and original but exceedingly verbose and lacking in critical discrimination or good judgment as to what should be included or omitted in his

compendium. . . . He seemed to me to be in the intellectual twilight zone between genius and insanity, and those of his own pupils with whom I happened to come in contact in the early days of my psychological experience were, it seemed to me, most of them cracked. Later experience taught me how profound his influence was upon some very brilliant minds; but it is useless to deny that his seminar has had a peculiar attraction for freaks who have done a good deal to make our science ludicrous in the eyes of sensible people generally. . . . Of course, when I met Hall personally and heard him lecture I recognized instantly the loftiness with which he towered above the level of American psychologists. He certainly belongs somewhere among the first five psychologists of our time, perhaps he should be ranked second.' . . .

"Enough about ranking. Hall was a pioneer. He was either '*a* pioneer' or '*the* pioneer,' as the table shows, in the opinion of a goodly number of his colleagues, in every one of the many and diverse fields in which he worked. This side of Aristotle or Leibnitz there is probably no other mind that so combines versatility and originality. Quoting from my helpers, 'Hall was the most desirable kind of a pioneer,—one whose work indicates the immense fields as yet untilled.' He dared to apply the methods of science to fields that had been sacred hitherto either from prudery or superstition,—witness his 'Adolescence' and 'Jesus the Christ.' 'I like to refer to his writings in spite of the fact that my advanced training was directed by —— and —— and ——. I always found them interesting and suggestive and always felt that he had taken me out on the frontier of psychological inquiry.' Two of our leading psychologists would pluck the plumes away. They designate this colossal worker, 'an organizer and popularizer rather than a pioneer.' But if the majority judgments were to constitute election to fame among the souls who dare to move out into new fields the following would be the true composite picture: 'He was a pioneer in his achievement and by nature; courageous, daring, resourceful, ever watchful for new trails and new ascents. He took large risks with eyes open; endured bafflements with stoicism and gathered strength by removing obstacles. And per-

haps more than this generation realizes, he suffered for part of his pioneer work and for his unflinching purpose to discover and publish the truth.'

"An index of the appreciative attitude of the members of this Association towards Hall's various contributions is found in the increasing weight of the voting on the several items as one moves towards the left of the table of values, that is, towards a gradation of higher worth. The numbers represent actual number of votes. The 'pioneer' columns mean in general rather higher valuations than do the ones designated as 'valuable'; for although the sheet bore the legend, 'You might check one of the first two columns and also one of the last five,' these careful-minded scientists for the most part did not notice that instruction and clearly considered the captions as a graduated scale throughout. The numbers pile up in general towards the higher rating. A little compilation would show that for the items taken as a whole there are, in the rough, six times as many votes for 'slight' contribution as for 'negligible,' four times as many for 'useful' as for 'slight,' twice as many for 'valuable' as for 'useful,' and twice as many for 'pioneering' as for 'valuable.'

"There is a vast scattering of judgments. Nothing Hall did was so excellent but that some psychologist was quite disgusted with it and nothing as you observe from the table so relatively poor but that many ranked it of highest value."

After giving illustrations of the diversity of opinion Starbuck concludes as follows:

"As I have tried to put my ear and heart next to the notes you have struck in this symphony of valuation with its profound harmonies and its Wagnerian discords there are some central motifs that need rehearsing.

1. "Hall is in the first place an *emancipated personality.* Few there be who attain deliverance. We earth-creatures have been only for a little moment or two, measured in terms of the earth drama, attaining some doubtful and dearly bought successes in pulling ourselves away from the enslaving contact with things and from our animal needs and passions so that we can

differentiate a few clear ideas and live by them. Man has had to struggle and fight his way to every inch of victory over the mere demands of his primitive nature. I like to contemplate the 'Adam' of Rodin. There he rises, that strong figure, towards an erect posture, that mighty child of world forces with strength in every sinew of his powerful frame and his earnest features not quite turned upward as yet away from earthliness, but beginning to clear themselves with the radiance of an idea. Contrast with 'Adam' the 'Balzac' by this same master sculptor,—his serene and steady features turned upward, this artist of the spirit who could portray in beauty with his pen the triumphing and triumphant life of mankind on the earth from which he had arisen. Every great soul must traverse the wide spaces from Adam to Balzac. One reads in the 'Confessions' of the New England lad, full of vivacity and 'insatiable curiosity,' pulling, tearing, bumping, and fighting his way out of the provincialisms and conventions of his early surroundings, towards greater contacts and higher freedoms. Through those long years of experimentation with life and its problems, meeting men and movements at home and abroad, he finally attained unto such a world citizenship as few achieve.

2. "A second theme closely related to the first is that Hall had by nature, or had discovered, the secret of perpetual growth. He was *persistently youthful*. There are at least three dead-lines that the elect of earth must cross after passing the period of infancy. The first of these is from thinghood to thoughthood,—not only to cross the line but to dwell beyond it, joyously and powerfully, in a world of ideas as in a temple of truth. Deliverance of that sort makes the true scientist and the genuine philosopher. The second dead-line is from thoughthood into a world of values. One who crosses this line can never be smothered under the trappings of his trade in science, cannot be a mere systematizer nor an architect of systems, cannot lose his vision. He gains what Plato was pleased to call 'a birth in beauty.' Hall crossed this dead-line and became one of the world's humanists. The third dead-line is that of approaching age with its slower metabolisms and sinister forebodings. Those few who safely cross this dead-line of aging have a

chance of a new lease of life, like a Tolstoi and a Browning, a new releasing of energies in the rejuvenescence. Hall swept away this boundary and lived with increasing buoyancy.

"It is to his credit and our gain that he did the rare thing of incorporating all these levels of animality and higher personality in the same individual. There is a way with men of turning their backs on the concrete facts of experience when once they gain a birth into thoughthood. They become mere dreamers, or old-fashioned philosophers with their feet off the earth. The worst offenders against life, as things go here below, are those who become emancipated into a world of meanings. They fly away into some abstract empyrean of spiritual values and become saints, or futurists, or dwellers in a seventh heaven of abstract values. Hall incorporated the whole range of life in one selfhood. He was even a healthy animal, full of human passions, good luck, that liked to play and work and make love; that could fight, making enemies and forgiving some of them, and with the spirit of a true sportsman letting the rest live; that had the instinct of ownership strong enough to prosper,—though guarding, perhaps, rather too jealously his scientific properties; with a native hunger of mind that ripened into a thirst for wisdom even though it gave way sometimes to a sheer love of acquisition. It is the part only of the rare genius to be facile and versatile in all the registers of the human scale.

3. "The third theme, which is but a variant of the last two, is that Hall is the *philosopher-psychologist*. A former Clark student whose experimental output is voluminous and significant writes, 'I was also helped personally by his wonderful philosophy of life which culminated so remarkably in his "Senescence." His general attitude toward the world and men made it possible for me to live and work effectively at a time when I surely would have perished without it.' Hall lived in a diverse and growing world, but one that was constantly striving for integration. These words from one who worked along with the President of Clark for some years will illustrate this truth:

I am pretty well set in the field of experimental psychology, and Hall's teaching and writing has been irrelevant to my field of competence. . . . Hall told his students in those days: "Build the top of the mountain first." And I, as a more matter-of-fact person, told them to start at the bottom. But I also felt that there were the two ways of working, and that it was a jolly good thing for the students to get both, and later, when they came to specialize, either. Hall was always bringing a broader perspective to my problems, and it was a delight to talk to him about them. But you see he had a sort of positive-negative relation to me, in that he took care of one aspect of the mental universe which needed to be taken care of and relieved me of it.

Hall himself always said to me that the great thing in psychology was the "synthetic" approach, and he encouraged me to be synthetic. I have never been able to define the term for him, but I have always intuited it to mean that psychology should not seek to prescribe limits for itself, but should understand the human being by drawing together all the various manifestations of human nature which make up, I should say, by far the larger part of contemporary knowledge. It was the exhibition of this conception which seems to me to be Hall's greatest contribution to psychology.

"Psychologists will naturally cleave asunder according to their admirations or depreciations of this philosopher-scientist who wishes to build the top of the mountain first. The factual- and practical-minded person cannot understand him—at least why he does it. A belated auditor came into the midst of David Starr Jordan's one-time famous lecture on the Ascent of the Matterhorn just at the critical moments of the description of privation, cold, and nearly fatal accidents. The bewildered late-comer soon leaned over and asked his neighbor in an audible whisper, 'what the devil were they doing up there,' and nearly broke up the meeting. Whether one likes personally the ozone and the sweep of vision or not is a matter of temper and temperament. The peculiarity in question would explain much. Did Hall have a prodigious memory for facts, titles of books and articles, and names of workers? Should one 'marvel at his assimilative erudition' and wonder at his 'encyclopedic knowledge?' He probably just saw things in perspective. 'Could there be,' one admirer inquires somewhat doubtfully, 'a third dimensional depth when there is such a two-dimensional sweep?' He saw. The view he got or glimpsed of a developing order

of life was so vast and interesting that his thoughts rushed and struggled for expression. 'I was both amused and awed,' writes one of his students, 'by his vocabulary. I laughed at and venerated him almost in the same breath because of it.' His thoughts came in such volume of suggestion, writes another, that 'it seemed as if he had three sets of vocal organs instead of one, they could not have given expression to his thought.' 'He was interested in those aspects of human behavior that are prolonged and not momentary.'

4. "The fourth theme of our symphony, important as it is in the ensemble, needs but the slightest mention. Hall is the *poet-psychologist*. Several persons have called attention to this trait with approval and appreciation. Others brand it mysticism, romanticism that ends in romancing, and the like. 'Hall's humanistic tendencies,' says one, 'are distasteful to me.' 'I think,' remarks another, 'that he is much more of a clear thinking literary man than an investigator and scientist.' A mature psychologist who studied awhile at Clark says, 'He viewed ideas with a sort of esthetic appreciation and he gave them back to his students in the same spirit. . . . As a discoverer of interests and values he was a master mind. I have never been able to agree with T—— in the position that science deals with facts not values. Hence I cannot see that Hall's place among scientists should be affected by this estimate.' This key-concept to right understanding is best described by the incomparably fine and sweet-spirited Sanford:[1] 'Dr. Hall was a typical Romantiker. . . . To him nothing human was alien. It was as a prophet and pioneer in these fields that he must be judged. To attempt to appraise him by other standards misses the essential meaning and purpose of his life.' It may be that the life and work of Hall is prophetic of the day when the world of values as truly as the sphere of facts and principles will be the subject-matter of science. Surely his later interests in the emotions had their drive partly from this conviction.

5. "The last point that may receive mention is that Hall's *values center in persons*. He was himself, first and foremost, a

[1] E. C. Sanford in *Amer. J. Psychol.*, 35, pp. 313 ff.

radiating and vitalizing personality. He was 'a vital contact,' 'you could talk with him.' 'He prevented more human shipwrecks than any teacher I have ever known,' writes one student, 'and always with a delicacy and respect for private affairs in his attitude.' 'He was always setting them (the students) on their own feet.' 'He did not teach me *how* to work or think or *what* to think, but he gave me the conviction and courage that one must dare to do his own thinking, that intellectual salvation grounds on that principle.'

"To record the fine impressionistic pictures my respondents have given of their contacts with Hall in the capacity of pupil and teacher would require a separate paper. I shall quote from but one more, which is representative:

> His great genius was at its best when he was taking young men of mediocre ability or even geniuses and imbuing them with a scientific point of view and spirit of research which they could never forget. I have never ceased to wonder at the marvelous results which he achieved in this respect. . . . His example and inspiration has not only enabled me to continue my scientific investigations but largely determined the way in which this work was done. It has made me turn to research at the times when I should really devote my time to recreation and rest because I would rather be able to make a few contributions by working overtime than to rest or enjoy myself in the ordinary way. Since my stay at Clark, whenever I have seen G. Stanley Hall his first question was always, "What are you now working at, and thinking about? What problems or research lie nearest your heart?" My answer was always matched with an equally free answer from him laying bare his most sacred new problems and insights. It is this spirit and attitude which all Clark men acquired from G. Stanley Hall that tends to make us devote every spare hour we have or can steal to research in a science which he made so all-alluring and inclusive.
>
> Another outstanding influence which worked wonders on all his students was his wonderful tolerance for the views and works of other scientists and psychologists. I never in my three years' stay at Clark heard him belittle any piece of experimental work. It was all important and interesting always.

"It has been said that the value of a great man is to lift the level of all men. We shall cherish the memory of Granville Stanley Hall not more for his sake than for ours. If by this season of contemplation of the fine personality and the signifi-

cant achievement of this earnest student and master teacher we are bound a little closer in the helpful fellowship that he sought to bring about through this Association, if we glimpse afresh the vastness and richness of the field in which we work, if we gain a bit of new perspective, if we are stirred within by the urge of possible achievement until we are more faithful to the smaller tasks and find strength to attempt the nearly impossible, it will have been good that we have met together."

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NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
VOLUME XII—EIGHTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

JOHN MASON CLARKE

BY

CHARLES SCHUCHERT

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1926

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John M. Clarke

"In the classroom of the venerable brown Albany Institute, Joseph Henry, a teacher of mathematics and physics, sent a galvanic current through a mile of copper wire and thereby created a circuit which, when broken, struck a bell. That little bell, a sacred relic, has sent its note around the world. That invention and discovery were the parent of the modern telegraph and telephone, the motor, the automobile, and the radio." The bell used by Henry in this experiment was secured by Clarke for the New York State Museum, and the portrait shows him with it.

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In the death of John Mason Clarke, America loses its most brilliant, eloquent, and productive paleontologist, and the world its greatest authority on Devonian life and time. Author of more than 10,000 printed pages, distributed among about 450 books and papers, of which 300 deal with Geology, his efforts had to do mostly with pure science, and he often lamented, in the coming generation of doers, the lack of an adequate appreciation of wondrous nature as recorded on the tablets of the earth's crust. He was peculiarly the child of his environment: born on Devonian rocks replete with fossils, in a home of high ideals and learning, situated in a state that has long appreciated science, he rose into the grandeur of geologic knowledge that was his.

Clarke is survived by his wife, formerly Mrs. Fannie V. Bosler, of Philadelphia; by Noah T. Clarke, a son by his first wife, who was Mrs. Emma Sill (née Juel), of Albany; by two stepdaughters, Miss Marie Bosler and Mrs. Edith (Sill) Humphrey, and a stepson, Mr. Frank N. Sill. Out of a family of six brothers and sisters, four remain to mourn his going: Miss Clara Mason Clarke, who, with Mr. S. Merrill Clarke, for many years city editor of the New York *Sun*, is living in the old homestead at Canandaigua; Rev. Lorenzo Mason Clarke, pastor of the First Presbyterian Church of Brooklyn; and Mr. William B. Clarke, managing editor of the Baltimore *American*.

The cause of Clarke's death was a malignant growth in the sigmoid, which did not cause distress until the last few months of his life, and was not discovered to be such until three weeks before. He died under the fourth operation on May 29, 1925. On the afternoon of June first, a throng of his friends and fellow workers assembled in St. Peter's Presbyterian Church to listen to its beautiful service and to pay their last respects. He is buried in the Albany Rural Cemetery, where also lie James Hall, Ebenezer Emmons, R. P. Whitfield, and Philip Ast, all members of the New York State Geological Survey.

The present writer's acquaintance with Clarke dates back to the autumn of 1889, when he also joined Hall's staff at Albany, but as his private assistant. Then ensued delightful Thursday evenings with Clarke and his family, when we discussed our favorite fossil brachiopods and exchanged remarks about the doings of our strenuous chief. What glorious evenings of inspiration these were! When the writer went to Washington, he still met Clarke two or three times each year and our letters were frequent, and this same close relationship was continued to the end. We saw each other for the last time at the 1924 meeting of the Geological Society of America at Ithaca, and Clarke's last letter, of May 14, 1925, was in his characteristic style, but with an undertone of possible seriousness ahead. The last few days before going to the hospital were, in fact, spent in adjusting official matters between himself and his staff, and in giving helpful suggestions to the Regents of the New York State Museum regarding its future welfare.

No finer thing can be said of John M. Clarke than has already been said by another close friend and associate, Doctor John H. Finley, former president of the University of the State of New York and now editor of the New York *Times,* from whose columns the following quotation is taken:

> He endeared himself not only to those of his own field of science, which has to do largely with the past, but also to scientists in every field and to the people of the State generally, because of his intelligent, practical and helpful interest in everything in the realm of nature under, about and above man's daily life. He was the best friend of the birds, the trees, and the wild flowers. He was concerned even for the migratory birds that flew across the State, and he followed them to their homes in the Gaspé or elsewhere to protect them there. And though a scientist of the highest standing, he still insisted that Pan is not dead. "I have seen him oft," he said, "among the forests of the mountains," and heard him "rustling through the iris swamps" or caught a glimpse of him "in the shadows of the salmon pool or coming down the lavender banks of evening." He had not only the scientific eyes to see beneath the surface of the earth, but the ears to hear the "reedy pipes" that "sing everything that can be sung" and "tell everything that can be told."

During the Sixty-first Convocation of the University of the State of New York, the evening of October 15, 1925, was devoted to memorial exercises for Doctor Clarke, the speakers

being the president of the University, Doctor Frank Pierrepont Graves, the Honorable William G. Rice, State Civil Service Commissioner, and Doctor Charles D. Walcott, secretary of the Smithsonian Institution. On this occasion, President Graves said:

In the death of John Mason Clarke the scientific world has lost its greatest paleontologist; the United States its most brilliant literary scientist; the State of New York its foremost champion of historic monuments, scenic beauty, and natural resources; the department of education its most distinguished scholar, its foremost intellect, and its chief ornament; and each of its members his most admired and inspiring friend. It is possible that his equal may some time in the future enter this building to serve the state and the cause of scholarship and science, but this good fortune is scarcely to be expected.

Secretary Walcott also said, in part:

New York State owes to Dr. Clarke an eternal debt of gratitude for preserving the records of its scientific activities of ninety years or more, and for developing and carrying on, as a part of the educational system of the State, a museum unexcelled among State museums. James Hall, the founder of the museum and the great scientific leader of New York State for fifty years, died with the impression that there was no one who would take sufficient interest to carry it forward as an educational factor in the life of the oncoming generations of the people of the State. He did not fully realize that the young man who had been associated with him for years had all the qualities essential to successfully sustain the work of the world-renowned State Geological Survey, and the upbuilding of a model State Museum. Dr. Clarke, by birth, training, ability, and spirit, was the ideal successor of James Hall, and it is to the honor and credit of the Regents of the University that they recognized his ability and fitness for the task and assigned it to him. Dr. Clarke effectively carried on a most important and valuable work, yet so quietly and modestly was it done that even those closely associated with him did not fully realize the contribution that he was making to science and to the reputation of the people of the State. His passing is a distinct loss to scientific interests in America, and to intelligent humanity throughout the world. The influence of such a personality extends through the medium of kindred minds to the men and women and to the boys and girls who are to be the future leaders and guardians of the material and spiritual welfare of the people of every nation.

PERSONALITY

Clarke stood about five feet nine inches, was sparely built, and probably at no time weighed more than 150 pounds. Dark

of complexion and blue of eye, he had a well balanced head and face, with a thin head of dark hair. In earlier life he wore a small mustache, but later on his face was clean shaven. Probably his most striking facial feature was his brilliant, alert, and flashing blue eyes, which portrayed much of the momentary feelings of the inner man; they were set in a face that even though rarely sad or stern, was not a smiling one. Ambitious, jealous of the New York Devonian, perhaps unduly suspicious at times, strong in likes and dislikes and quick of temper, he was easily aroused, and his face, and especially his eyes, changed with his mood. However, he usually had these phases of his make-up under good control. He was a great tease, and particularly brilliant in the company of ladies, delighting in the give and take of repartee, in which he was rarely outdone. He was richly endowed with imagination, which had its full play in his last book, "L'île Percée" (1923). Of sarcasm he had much, and woe unto the man who angered him into action! Always well groomed, eloquent in speech, pleasing in manner, and deliberate in action, he moved easily in society with the unconscious ease of one to the manner born.

Mentally, Clarke was brilliant, alert, orderly, and well trained. Always a lover of the worth-while in life, the beautiful in nature and art, and ever a hard worker, he quickly became a prodigious producer of excellent paleontologic and stratigraphic results. These qualities made of him also a collector of antique ceramics and furniture, and historian of the fisher-folk of Quebec, and they were reflected even more strongly in the unusual character of the museum under his direction, which is probably his best monument.

Clarke's ideals in science were of the highest, and his leanings were decidedly toward pure science rather than the applied aspects. At times he was very outspoken in this matter, lamenting that so few geologists nowadays go into science for the love of the work, and that most men take up the subject as a means toward a better living. This will of course always be so in most instances, and yet there is no denying that the tendency in the United States has long been alarming, and especially since 1914. Ever since the Great War our universi-

ties have found a marked dearth of good men to take up teaching, and the surveys look in vain for leading geologists and paleontologists. Accordingly, to make himself felt in this matter, Clarke at times overstated the situation, giving rise to ill feelings. On the other hand, he fully understood that the mining world can pay higher salaries to good men than can the universities and surveys, and yet it can not be said of him that as director of the Geological Survey of New York he neglected to develop the natural resources of the State. Certainly he did for the mining and engineering worlds ever so much more than did his predecessor.

Even though the New York State Survey and the Museum have long been under the guidance of the Regents of the University of the State of New York, the appropriations for their maintenance have to come through the state legislature. Therefore both Hall and Clarke have seen much of the regulation statesmen and their humorous, if not always wise, ways of doing things for science. Hall rarely was the gentle mixer with people, but his great prestige in science generally assured him success with the legislature. Clarke, on the other hand, had the gift of making good addresses and of meeting easily men of importance, and this, together with his caution in keeping abreast of coming events among the powers that are and are to be, made him a tower of strength behind the Regents. He was therefore much more successful in making things possible in the Science Division of the University than any of his predecessors.

Ancestry

During the years previous to 1892, Clarke was searching out his family history, and in this year he completed the task and transcribed the information by hand on twenty-five cards 6 x 3 inches that were later bound into a leather-covered booklet. Before it was finished he showed it to me one Thursday evening, and I now have this treasure again in my hands. Illuminated and embellished with tail pieces and with two signatures of the ancestral Clarke, and with two well executed full-page line drawings of the New Hospital at Plymouth, Devon, and of the site of Lieutenant Clarke's house in Northampton, the three

chapters and three appendices of the booklet are done in the style of composition and spelling of the early Colonial days, forming an interesting example of Clarke's painstaking care and artistic skill, wrought out for the eyes of his family only. It is entitled "A Fragment of the Life of Lieut. William Clarke, Puritan and Pioneer: An Early Member of the Massachusetts-Bay Colony settling on the Dorchester Plantation and afterward removing to Northampton. . He lived an Honoured and Useful Life and founded a Large Family in a New World: To a few of his Descendants these pages are indited. The Tuyck, 1892." Much of what follows is taken from this booklet.

On the twentieth of March, 1630, there sailed from Plymouth, in Devon, the ship "Mary and John," a vessel of 400 tons, with 140 men and women of the East Anglian yeomanry, bound for the Charles River in the Colony of Massachusetts Bay. They were set ashore at Nantasket, now Hull, on the thirtieth of May, 1630, and shortly afterward most of them located at Dorchester Plantation, the second oldest settlement in the colony.

In this party was William Clarke, of pure English strain, although nothing further is now known of his ancestry. It is, however, the booklet adds, "fair to presume that the subject of this sketch was a young man of honour and probity, whose mind had been profoundly moved by the theological controversies of the age. . . . In his days he was a tiller of the soil, though withal a vigorous man of affairs."

There is no other record of William Clarke until June 23, 1636, when his name appears on the church records at Dorchester, together with that of his wife, Sarah, who was probably also of the "Mary and John" party. They lived at Dorchester for twenty-one years, tilling the land and raising a family of ten children. Then all of them moved to a newly arisen center that soon came to be known as Northampton.

The journey from Dorchester to the Connecticut Valley, made in 1659, was a plunge into an Indian-ridden wilderness. Mr. Clarke was then fifty years old, and made the way on foot, his wife riding horseback. "Panniers slung across the horse held each a baby, one the little William (whence our line is derived), the other the infant Sarah." The journey accomplished, Mr.

Clarke took possession of the twelve acres assigned him by the selectmen in the western part of the town on land which is now occupied by a part of the Smith College buildings. Here he built a log house, and later, on the river front, grist and saw mills, and here he lived for twenty years.

Life in the little settlement of some forty persons "was not only primitive and agricultural, but dangerous as well, for the savages did not treat these settlers with the uniform kindness shown to those at Dorchester, but their murderous onslaughts were disturbing and not infrequent. The town was at once obliged in self-defense to organize a military company, of which Mr. Clarke was chosen Lieutenant, the highest secular honour in the power of the little hamlet." Other honors came quickly, and for seventeen years he was representative at the General Court at Boston, and justice of the peace for a long period.

Lieutenant Clarke's descendants prospered, and the line of seven generations terminating in Noah Turner Clarke, the father of the subject of this sketch, was prolific, each generation furnishing about ten children. The line proceeds through five successive William Clarkes, spreading from Northampton to Lebanon and thence to Windsor in Connecticut.

William Clarke the fifth was a member of the Continental Congress, fought at Lexington and at Bennington, and after the Revolution moved by oxcart to the "Seneca Country," where he was one of the founders of Naples. It is interesting to make a slight digression here to note the cause for this migration. All of New York west of Seneca Lake originally belonged to Massachusetts, but she sold her rights to one million acres to Oliver Phelps and Nathaniel Gorham. They in turn sold these lands to the veterans of the Revolution, and to citizens of Massachusetts and Connecticut. Thus it came about, as Clarke says, that "these early villages were bits taken out of the Puritan atmosphere and set down in western New York," where Canandaigua and Geneva became "dignified centers of education and refinement."

The ninth child of William Clarke the fifth, born at Windsor in 1787, was Lorenzo Clarke, whose second child, born at Naples on April 8, 1817 (died 1898) was Noah Turner Clarke.

Noah Clarke married Laura Mason Merrill, and they had six children, of whom John Mason was the fifth.

In the booklet above referred to, Doctor Clarke writes further: "There was another man aboard the 'Mary and John' in whom we (I say this to my next of kin) have an equal interest. I mean Mr. (afterward Captain and Major) John Mason, some time resident of Dorchester, later the doughty fighter in the Pequot Wars." He soon moved to Windsor, as did the Clarkes, and from the family which he founded came Laura Mason Merrill, the mother of Doctor Clarke. Major Mason was born in England about 1600, came to this country, as we have seen, in 1630, played a prominent part in the struggle against the Pequot, founded the town of Norwich, Connecticut, and died there in 1672.

John Mason the second, we learn from data furnished by Noah T. Clarke, married in 1786 Sally Woodward, direct descendant in the fifth generation of Governor Bradford of Plymouth Colony and Alice Southworth, his wife. He had eleven children by Sally Woodward, of whom the fifth was Laura Mason, born at Castleton, Vermont, in 1796, and married in 1819 to Selah Higley Merrill, a lawyer of Castleton, son of Enos Merrill and of Delight Higley, a scion of the Brewster family of Plymouth. Mrs. Merrill died on July 9, 1820, four days after the birth of her daughter Laura. The child was brought up by her mother's sister, Altha Stevens Mason, who was the widow of Aaron Dana and who eventually went to live with the Clarke family at Canandaigua, dying there in 1880 at the age of eighty-six.

Another aunt, Sarah Mason, married Gideon Davison of Saratoga Springs, and Laura Merrill spent part of her girlhood in that village, going to school there. A third aunt, Margaret Fanning Mason, married Henry Howe, who became resident principal of Canandaigua Academy, and with this family the girl Laura resided later, going to school at the Ontario Female Seminary. In 1846 she was married to Noah Turner Clarke, then associate principal of the academy at Canandaigua. She died in that village on November 5, 1887, her husband and all her six children surviving her.

Youth and Rise into Geology

John Mason Clarke, the fifth in a family of six children, was born in the beautiful lake resort of Canandaigua, New York, on April 15, 1857. His father, Noah Turner Clarke, was for fifty years a teacher of science, and for nearly thirty years principal of the Canandaigua Academy. The boy John not only got his first schooling in the Academy building, but was actually born there, since his father did not begin to erect the Clarke homestead until 1858, and occupied it first in 1859.

Noah Clarke, at the age of twenty-one (1838), got his first experience in geology from James Hall's field assistant, Eben N. Horsford, who was born and raised in the Genesee Valley. "The latter found in Clarke a young school teacher of just his own age, a native of the place, interested in everything out of doors, and to him he appealed for guidance through the gullies and over the hills of the region. . . . The young schoolmaster afterward became a student in chemistry for one year under Horsford at Harvard" (Life of James Hall, p. 74). In 1870, Noah Clarke found a nest of early Upper Devonian crinoids, one of which Hall named after him (*Melocrinus clarkei*). The Clarke family, therefore, had long been interested in the geology of New York, in fact ever since the organization of the State Geological Survey by Governor Clinton.

Of Clarke's boyhood, his sister, Miss Clara Mason Clarke, has this to say:

> It seems to me John was always picking up "stones." When our home was built, the yard had just been filled in and "cowhorns" and "shells" were numerous. As a lad he used to trudge off with his bag and hammer to investigate some gully or lake shore. . . . He was particularly fortunate in finding trilobites, and my sharp-pointed black pins were greatly in demand for picking away the rock, so that the trilobite should stand out as it originally was.

His younger brother, Rev. L. Mason Clarke, adds:

> When he and I were not more than seven and five, respectively, we were interested in cutting out pictures of all sorts of animals which came with the big circus and menagerie posters, then in common use.

We arranged them in match boxes, the long, old-fashioned blue ones, and tied the boxes with string and drew them along the hall floor as a procession, and John would learn all he could about the varieties and discourse more or less to me about them. He began his first "finds" in our old garden. John would hoe up spear and arrow heads. . . . He was always very sharp-eyed and could see something where I could not. . . . Of course Father was a good deal the same way and John found considerable encouragement from Dad, who as a surveyor was usually digging up something curious in the way of fossils. John was ever collecting; he had it in his system. . . . He always was a leader in what he undertook, even as a child. Born a Nature-enthusiast, yet even as a child he was always alert for other things.

Fortunately for us, Clarke himself wrote out the history of his earliest days, working on this at different times, particularly in 1904 and 1917. From this account we learn that shortly after his father moved into the new home a well was dug, and on the pile of Hamilton shales thrown out young Clarke collected his first fossils. He got in abundance "cowhorns" (cup corals) and "shells." He goes on to say:

I can see now the blue paper match boxes which contained these treasures lying on the shelves of the closet off my sleeping room. Too young for a teacher or for an intelligent appreciation of these things, an instinct seemed to guide me in their acquisition, and after every ploughing of the large garden I forked over the soil searching for such fossils as the loose stone would afford, and over the gathered heaps of stone which had been brought together to clear the soil. I presume my blue boxes date back to a time when I was not more than seven years old, but I must have been ten when my father, who encouraged every impulse in me in this direction, put me in possession of Hugh Miller's writings and Hitchcock's "Text-book of Geology." The texts were too much for me, but the pictures in Hitchcock were an inspiration. In it on one page was the picture of a "Silurian trilobite," on another a "Devonian trilobite." My father's house was built on a high cellar wall made of large field stones, crystalline boulders and blocks of Coniferous limestone. The corner stones were of larger size and all of the latter kind, full of the fossil remains with which that rock abounds. I could never in later years get my father to remember, though clear enough to me even now, a day when I eagerly called him to one of these corner stones to show him where I had found a "Silurian trilobite" and a "Devonian trilobite" in the same block!

In 1923, in his eloquent "L'Île Percée," Clarke comes back to the Hugh Miller of his youth:

Hugh Miller's books were my earliest guides in geology—I will not say inspiration, for some of them were pretty hard reading for a boy, but after all he made his odd fish rather attractive by his extraordinary descriptive powers and his trenchant English style. . . . My debt to him was a personal one.

To go back now to the 1904 account:

In a desultory way as opportunity afforded between the later demands of school, and as the inspiration caught me, though laid aside at times, my interest in this collecting of fossils continued and became a productive pursuit as soon as I had learned how to make use of the volumes of the Paleontology of New York. [This was in the early seventies.] My home region was most beautifully supplied with the objects of my search, the shale cliffs along the shores and the ravines of the lake teemed with fossils, and I was in great measure solely a creature of my environment.

The region of Clarke's home is in the type area of the New York Devonian,

the simplest, most lucid and most complete development of one great geological system, the Devonian, that the world has ever revealed. . . . The rocks of the country overflowed with fossils, often in beautiful preservation. They showed themselves in the stone fences and farm foundations; they lay loose along the streams and on the shores of the Finger Lakes; and they protruded from the rocks on the edges of the cliffs. So ubiquitous were they that the Seneca Indians used the fossil cup corals for pipes, strung together the joints of crinoid stems into necklaces, and buried brachiopod shells along with axes and spear points in the graves of their brave.

Beside the daily strenuous demands of school these natural proclivities had no standing and were pushed into the background. But the break in the routine permitted me to indulge a growing interest in the mollusca of the region, and before leaving for college I had got together the mollusk fauna quite completely, both of lake, stream and woodland. When my good father sent me [1874] in turn to college at the cost of all he could get together, for there were four of us in succession, my zeal lay dormant or was for a while snowed under by the Latin and Greek and mathematics of the old-time course. I would not to-day surrender the little I have retained of the former for a good slice of my store of science. Not till I arrived at junior year and the course of gentlemanly and refined discourses on mineralogy by Professor C. U. Shepard in which the students were allowed to look at but not to handle specimens, did my suppressed love break out in full force.

In his "Sketch" of 1917, he says:

Entering Amherst College in 1874, I fought bravely against the handicap of a poor preparation in Greek, warmed reasonably to my Latin, was dead in mathematics;. . . . it was not until I reached the courses in geology, mineralogy and zoology given by the distinguished Professor Benjamin K. Emerson, that my love for natural science, born long before, had a new birth. . . . Thus I got my start and left college [at the age of twenty years] with the class of 1877, the best of its kind.

The first year out of college [1877-1878] was on the home ground, teaching in the Academy anything that came my way and diligently pursuing the study of the local geology. I had now become methodical. I collected from the Hamilton rocks with the greatest diligence and in profusion, plotting the vertical range of every species and these to the number of 300-400.

It was in this year that Clarke met D. Dana Luther, an amateur geologist living at Naples, of whom he goes on to say:

Like interest in the mysteries of the rocks brought us together. One the staid, wise and cautious man, and the other a young, trained enthusiast. One had nothing else to do and both everything to learn. With his acquaintance came long summers together of excursions into all the rocks of western New York.

Continuing the discussion of his stratigraphic work, he says:

On this basis I sought to establish a zonal division of these strata by faunules. . . . It was the first attempt of the kind, I think, on the Paleozoic rocks of New York, and though the homogeneous Hamilton shales did not well lend themselves to this effort, yet in a fair degree this end was obtained.

Clarke did not begin to publish these stratigraphic results until 1885. In the meantime, H. S. Williams had been appointed to the chair of geology at Cornell (1880) and his publications of 1881-1882 show that he also was studying fossil faunas as units for investigation in the Ithaca region. It therefore appears that these two paleontologists independently discovered the value of zonal stratigraphy, though Williams, who was ten years older than Clarke, got into print about three years earlier.

In the autumn of 1878 I was called back to Amherst as assistant to Professor Emerson, and part of my work while there was to arrange the great C. U. Shepard collection of minerals which was next year destroyed by fire, and also the extensive C. B. Adams collection of Mollusca, out of which I culled an extensive familiarity with molluscan species and prided myself somewhat on the fact that I could then recognize and give the names of a thousand species at sight. While thus

engaged I wrote my first paper in a scientific journal (American Journal of Science) upon the discovery of a rare molluscan genus [Gundlachia] in western New York.

Now burning with the fires and fairly well along in my training, I returned to Canandaigua Academy for another year [1879-1880] of teaching and of geological work in the country side. My collections had grown to commanding proportions and ever crowded upon the space available for them in the old home.

In the autumn of 1880 I became teacher of science in the Utica Free Academy, succeeding the late George H. Williams, my collegemate and co-worker at Amherst in graduate work. Then in 1881-1882, on the recommendation of Professor Emerson, I was invited to go to Smith College as instructor in Geology, Mineralogy, Zoology and Botany. I demurred to the botany, and to the salary. Of the former I knew but little and the latter was no improvement on that in hand. The trustees met my objections most freely and thither I went. . . .

At my second year there I was made professor, and the end of this year [1883] I retired, having determined to wait no longer for my long hoped for experience in Germany. My withdrawal was regarded as temporary and with expectation on part of the president and myself of return.

In the summer of 1883 I went to Göttingen and joined Von Koenen, and returned in October, 1884. These days made history for me and will ever be filled with happiest recollections. I returned to Northampton to give the instruction in geology which had been cut down during my absence and the position curtailed by the establishment of a professorship of biology.

Just before Clarke was ready to return to America, I learn from his son Noah, he received a letter from President Seelye of Smith, accusing him of heterodoxy, and closing with the statement that Smith did not require his services longer. This was a direct violation of Clarke's agreement, but what was he to do under the circumstances? The change cut short his hopes of continuing his studies at Göttingen and of obtaining the doctor's degree in course. During the school year 1884-1885, therefore, Clarke gave instruction in geology, zoology, and German at the Massachusetts Agricultural College at Amherst. Then followed days of waiting at Canandaigua, where he continued to work on the Upper Devonian (mainly the Naples fauna), a study begun in the summer of 1877 and intended as his dissertation at Göttingen. Out of employment, he appealed for work to James Hall, whom he had known since 1875. His

persistence was rewarded, through the strenuous assistance of a state senator, and on January 1, 1886, he became assistant to the man who was then the master paleontologist of the country.

Clarke's entry into Hall's service is described in his "Life of James Hall," as follows:

> In the autumn of 1885, I came to Albany partly to attend, so far as I might be permitted, a meeting of the National Academy of Sciences . . . but more particularly to show to Professor Hall and Mr. Beecher a quantity of new things in trilobite and crustacean lines which I had been extracting from the Devonian rocks in western New York. . . . The trilobites made so effective an appeal that I was to join the force at once and trust to good luck to edge my way into the service. On the first of January, 1886, nine o'clock in the morning found me in front of the great stove in Mr. Hall's "office" on the Beaverkill, trying to dry my soaked clothes after a rough tramp of two miles through a foot of freshly fallen snow. No one else was there; but presently the ruddy Santa Claus of Hall's figure coming in from breakfast appeared through the door, and with a gray look of surprise in his spectacles as he saw me by the stove, he said: "Oh, yes. How do you do? Could you lend me two dollars?" Then began my association of twelve years with this extraordinary man [then 79 years old] whom I had known slightly for ten years past but whose equations I had yet to learn.

The office assigned Clarke in the State Hall he occupied for twenty-five years, until he moved into the Education Building.

Museum Career

From January first, 1886, until his death in 1925, Clarke was connected with the Geological Survey of New York, rising to the position of State Paleontologist in 1898, and in 1904 to that of State Geologist and Paleontologist and Director of the State Museum and of the Science Division of the Education Department. From 1894 on, he was also professor of Geology and Mineralogy at the Rensselaer Polytechnic Institute in Troy. Of these appointments, that to the directorship of the Science Division was the most significant one in his life, since now he had not only inherited Hall's official mantle, but added a great deal more. As he says:

> This position involved not only the State Museum, but as well the research departments in Entomology, Botany, Zoology, and Archeology. My time had now to be divided between my special scientific interests and the creation of a new State Museum.

In 1907 and 1909, Clarke was also working toward a State Historical Museum, and his plan for it was distributed to all the historical societies in New York, and to others. Its growth was to come gradually through the present museum, and a beginning was made with Ethnology. Shortly afterward, a gift of $15,000 from Mrs. F. F. Thompson made possible the installation of the "Governor Myron H. Clark Museum of Iroquois Ethnology," now represented in the Museum by six large cases showing the family and religious life of these Indians in a manner unsurpassed elsewhere. In 1908 the Museum was made the keeper of all the wampums of the Iroquois Confederacy, and the director was given the title of Ho-san-na-ga-da, "Keeper of the Name."

The needs of the rapidly growing museum and of the other divisions of the University brought about the construction of the great Education Building, which was completed in 1913. In the autumn of that year, the Museum began to move into its new quarters, which occupied the entire upper floor, with 60,000 square feet of space. One-half of this space is devoted to Geology and Paleontology, an expansion that brought about an increase of staff and a modernization of the grand collections. The dedication of the building took place on December 29, 1916, before the assembled geologists of the country, who were addressed by Theodore Roosevelt. This was the proudest day in Clarke's life.

New York now has the best state museum in America, with the finest array of highly significant Paleozoic fossils. In invertebrate paleontology, it possesses one of the world's most valuable collections, containing upward of 7,000 type specimens, and constituting a mecca to which all students of the older Paleozoic go for inspiration and interpretation. Clarke also inaugurated the plan for the very artistic and lifelike restorations of Ordovician, Silurian, and Devonian marine assemblages of New York, now shown in the Museum, and this type of installation and teaching had its culmination in February, 1925, when he placed before the public a reproduction of the Gilboa Devonian forest, a living picture of the first flora to clothe Mother Earth. The creation of this attractive State Museum

Clarke regarded as his chief contribution to the civic and educational development of his State.

Clarke also planned and put into execution, with expert aid, the splendid volumes entitled "Birds of New York" and "Wild Flowers of New York," both of which have done great service to the people of the State, especially through their widely distributed cheaper editions. He established, with the support of the president of the University, the annual State Bird Day, and the annual courses of free public lectures given in the Museum.

Another service to the State which Clarke brought about was the acquisition of "unique or remarkable bits of geological scenery of high educational value . . . endangered by the progress of settlement or industry." He began this movement for the protection of natural monuments in 1908, and his first result came five years later with the gift by Mrs. J. B. Thacher of 350 acres to establish Indian Ladder Park, west of Albany, where the actual base of the Devonian can be studied to best advantage. Soon followed Lester Park, near Saratoga, showing in a striking way the Upper Cambrian algoid ledges; the Myron H. Clark Reservation, a glacial park near Syracuse, showing, among other effects of glaciation, the making of a wall over which dropped falls that were nearly as grand as Niagara; and the Starks Knob Reservation, near Schuylerville, with its long extinct volcano. There are now six of these parks or reservations under the administration of the New York State Museum.

Clarke dreamed of a yet greater state museum, a central one covering the entire field of museum interest within the scope of the State, with full supervising control of local museums. The statutory conception of a New York State Museum, he said in 1913, is "any public museum which the people of the State may choose to bring into existence, whether it be a museum of history, of art, of industry, or of education." It was, however, not to be a world or metropolitan museum, but one solely devoted to the things of the State. Clarke's plan for this great conception was completed in 1916, and presented to the assembled geologists at their Albany meeting, and in 1922 the Regents had received favorable consideration from the State

Roosevelt Memorial Commission. But his dream, when its fulfillment seemed almost at hand, was for the time set aside by the legislature of that year, when it voted two and one-half million dollars for a Roosevelt Memorial to be connected with the American Museum of Natural History in New York City. This act of the legislature broke Clarke's administrative spirit, and ever afterward he failed to show his former strength of planning and the dash so characteristic of him. As Walcott said in his memorial, "He felt most keenly and grieved to the end of his life that the effort to deprive the capital of the State of a great memorial to its former governor and foremost citizen of his time, Theodore Roosevelt, should have been successful."

State Survey

While Clarke had charge of the Geological Survey of New York (1898-1925), with the aid of a competent staff nearly one-half of the State was geologically mapped on the scale of one mile to the inch, and the mineral wealth exploited. Summoning to his assistance the best experts available, and issuing their results in something like seventy bulletins, he brought the knowledge of New York geology, its rock structure, its marvelous fossil record, and its mineral industry to a condition that makes it safe to say that no equal area of 50,000 square miles in the world is so completely known and understood. Among those who did most of this work may be mentioned D. Dana Luther, James F. Kemp, H. P. Cushing, Rudolf Ruedemann, W. J. Miller, H. L. Fairchild, H. P. Whitlock, D. H. Newland, J. B. Woodworth, A. W. Grabau, G. H. Chadwick, and others.

Gaspesia

No sketch of John M. Clarke, man or geologist, would be complete without recording the second great passion of his life, that for his beloved Gaspé. Attracted first to this far eastern portion of Quebec by the promise of geologic riches, he found besides a land of vivid beauty, discovered by Cartier in 1534, and peopled by fisher folk whose simplicity of living and traditions of hospitality spell peace to men weary of the strife of

the market place. It was not strange, therefore, that he returned to Gaspé summer after summer, tramping its shores for three hundred miles, and living among the people of the coast, so that few knew the country, its human records, and its natural history, as he did. His interpretation of the geology is dealt with later in a discussion of his scientific work, but here we may pause to mention the two books in which his mastery of style finds itself at its best—both born of Gaspé. Few regions of the earth have had so ardent an interpreter, in whose pages the wonders of the geologic past, the marvelous beauty of land and sea, the stately days of the Seigneury, and the quaint customs of the present day mingle with equal warmth and color. "The Heart of Gaspé," published in 1913, and the even more charming "L'Île Percée" of ten years later constitute an important contribution to literature as well as to history and science.

In studying the history of Gaspé, Clarke helped to locate the site of the old French Customs House occupied by the Intendant, M. Revolte, and destroyed by Wolfe in 1758, and to dig out from the sands of the peninsula where it stood the relics of the old régime now in the Chateau de Ramezay. From Cap des Rosiers he took the Cartier medallion, and from Bonaventure Island he had the old freebooter Duval's cutlass with its gold plated brass mountings, and its beautiful damascened and inlaid blade, carrying the royal arms and the monogram of George III heavily worked into the hilt. Later he felt that the latter reminder of pioneer days should remain at Percé among the old families who knew the local history, and so he gave it to the leading merchant of the village, Mr. Charles Biard.

Clarke's interest in old china also found a new field in Gaspé, and, as he says, "led to the acquisition, at a time when no one else was interested, of a large collection of the old wares of the people." His publications on ceramics include "English Gold Lusters" and "the Swiss Influence on the Early Pennsylvania Slip Decorated Majolica," both privately printed at Albany in 1908.

Clarke actively concerned himself to arouse provincial and federal interest in the preservation of the waterfowl nesting

places on Percé Rock, Bonaventure Island, and the Bird Rocks of the Magdalen Islands off Gaspé. He visited the Magdalens frequently, and wrote accounts of their geology and history. In recognition of his interest in Gaspé, the Quebec Board of Geographic Names in 1917 attached his name to a township covering the upper reaches and great salmon pools of the Grand Cascapedia River.

Other Public Acts

Clarke's strong historical sense is again seen in his placing of memorial tablets. This began in 1901, when he and a few of his associates placed on the home of Ebenezer Emmons on Hudson Avenue and High Street in Albany a tablet commemorating the fact that in this house in 1838-1839 was started the Association of American Geologists, the parent body of the American Association for the Advancement of Science. In 1908 he placed a tablet in Letchworth Park near the Portage cataracts on the upper Genesee to commemorate the first geologic work done by James Hall in western New York in 1839-1843. Five years later, through his efforts, Logan Park was set aside in Gaspé, and here he unveiled before the geologists of the Twelfth International Geological Congress a bronze tablet memorializing Sir William Logan's pioneer field work in eastern Canada. In 1916, at the meeting of the Association of American State Geologists, a memorial tablet was placed on Hall's private museum in Beaver (now Lincoln) Park in Albany. The grandest memorial of all, however, is to be unveiled this autumn (1928) in front of the old Albany Institute during the convocation of the University of the State of New York—a large bronze statue of Joseph Henry, the first secretary of the Smithsonian Institution, who was a native of Albany. Clarke started the movement for this memorial in 1916, raising $25,-000, and he was to have made the unveiling speech.

Clarke was always much interested in the welfare of the city of Albany. Here he did much to rehabilitate the Albany Institute, one of the oldest scientific societies in the country, and was its president for many years. He was also president of both the Historical and Art societies, and they thrived greatly

under his care, as did the Burns Club and the Walter Scott Memorial Association. He helped and cheered on the good work of the ladies of the Dana Natural History Society; was trustee of the Schuyler Mansion, and from 1916 on, of the Dudley Observatory. A member of the Fort Orange Council of the Boy Scouts, as well as of the National Council, he arranged for the Mayflower Medal to be awarded each year to the scout showing the best knowledge of the local history.

Clarke's championship of the need of preservation of scenic beauty was best exemplified in the case of Niagara Falls; as he so well says:

> The claim of the higher life, the demands of the finer emotions, the love for the beautiful in nature, express themselves in part in the government protection of natural wonders from defacement and destruction; in organizations created to keep alive this sentiment and extend the ægis of the State over natural glories which belong to mankind rather than to men. No wise man confesses himself devoid of such emotions. The violation of this principle in present practice offends the best sentiments of the race.

The "menace to Niagara" resulting from the desire of the power companies to utilize the falls, Clarke was one of the first to point out. The danger had long been threatening, and in 1904 the fight culminated in the New York Assembly. Clarke stood out against it, and in public addresses and otherwise pointed out that—

> The conservation of Niagara Falls is a question of public morals. About 800,000 tourists visit the Falls each year, and their number demonstrates how closely the interest of the whole world is focussed on Niagara, for these visitors are representatives of every nation. How many hundreds of thousands will seek out Niagara when the world learns that the Delilah of commerce has shorn it of its glory? Will they traverse the seas to behold the wonders of a breakfast-food factory, or any other industrial triumph? These are everywhere; Niagara is unique.

This battle, in which Clarke played so large a part, was won by the conservationists, and a treaty has been made with Great Britain, keeping the water of the Falls under reasonable control.

In 1902, Clarke was delegated by the Geological Society

of America to represent that body at the dedication of a memorial to Hugh Miller, the Scotch quarryman, newspaperman, and geologist, who had been in some measure the inspiration of his youth, and for whose memorial—the Hugh Miller Museum at Cromarty—he had raised a considerable sum from the Scot's American admirers. At a later day, and as an appropriate cis-Atlantic memorial to Miller, Clarke proposed the name "Hugh Miller Cliffs" for the Devonian fish beds at Migouasha, a name accepted by the Quebec Geographic Board. Of these cliffs Clarke writes in "L'Île Percée":

> The Hugh Miller Cliffs are planted on French Catholic soil but their face looks sternly out over the river to Scotch Presbyterian New Brunswick. There one may stand on the shores where the salmon now run and from the rocks extract their odd, heavily armored ancestors, which might have died of their mongrel Greek names if a merciful nature had not put them to a kinder death. Among them was the magnificent Eusthenopteron (*pardi!*), which is as fine a salmon as the "Auld Red" ever produced. Hundreds of specimens of these ancient fishes have been taken from these rocks and thousands remain for him who will search them. My hammer never touched a rock with a more inspiriting impact than here. Fishing! At every "cast" of the weapon some fish rises, or mayhap a fern-leaf gets tangled in the tackle. And this is the sort of fish one can ship to his friends without hurry or ice; they will not spoil after their millions of years of storage.

Biographies

With a strong historic turn of mind, it was but natural that Clarke should be an excellent biographer. This is only too apparent in the sketches which he has given us of G. H. Williams, G. B. Simpson, C. E. Beecher, J. C. K. Laflamme, R. P. Whitfield, N. H. Winchell, H. C. Hovey, C. S. Prosser, W. B. Clark, James Eights, H. P. Cushing, James Hall, and Archibald Geikie. The tribute to his old chief, published in 1921, a book of 565 pages, is in a class by itself. Hall was a youth of 25 years when he entered the service of New York and seven years past fourscore when that service ceased. In the biography, Clarke portrays "the man as he was; the influences that guided him and that he imparted; the work he did and the manner of doing it; the friendships he made and the esteem he won." Written in an animated style, eloquent in diction, replete with

humor and anecdote, it shows us not only what manner of man Hall was and the tremendous amount of work he did, but also the men with whom he worked and those with whom he came into direct contact. It is, in fact, one of the two best contributions to the history of North American geology and paleontology, set in a background of these sciences in Europe. Reading it, we are forced to the conclusion that New York State truly has been the "mother of geologists." My review of this book in *Science* pleased Clarke, and he wrote me in one of his letters: "If you don't get your reward on this earth, I'll try to arrange matters with St. Peter so that we can sit together."

HONORS

Of "honors which beautify and crown success," Clarke had many. Four times he was called by leading universities to take their chairs in geology, and each time with a better salary than he received at Albany (Ohio State 1898, Columbia 1900, Yale 1904, and Pennsylvania 1919), but each time he declined, saying that his work was in New York. To his own commonwealth he gave his best as a loyal son, and he was deeply grateful for the liberal support that she gave him in return, as appears again and again in his annual reports to his superior officers.

Clarke was elected to something like fifty scientific and historical societies in this country, Canada, England, Germany, France, and Russia; made an Immortal in the National Academy of Sciences in 1909; elected to membership in the American Philosophical Society, the American Academy of Arts and Sciences, the Geological Society of London, the Authors' Club of London, the Société Russe de Minéralogie. He was vice-president of the Geological Society of America in 1909, and its president seven years later; first president of the Paleontological Society in 1909; received the Prix de Léonide Spindiaroff of the International Geological Congress in 1910 for his work in Gaspé, a gold medal from the Permanent Wild Life Protection Fund in 1920, the Hayden Gold medal of the Philadelphia Academy of Natural Sciences for excellence in geological re-

search in 1908, and the Thompson Gold Medal of the National Academy of Sciences the month before his death.

Of honorary degrees he also had many. Most signal of these, in his own opinion, was the honorary Ph. D. conferred upon him by the University of Marburg in 1898, "done without my knowledge and done so rarely that not half a dozen such degrees are conferred annually by all the German universities together. At the time it was given no American known to me and none now [1904] bears the same degree." Colgate gave him an honorary Sc. D. in 1909, Chicago in 1916, and Princeton in 1919. From his Alma Mater he received the LL. D. in 1892, and from Johns Hopkins in 1915. To all these honors was to have been added another, since letters from Professor Barrois of Lille state that he was to have been made a fellow of the French Academy at its next annual meeting.

PUBLISHED WORK

Paleontology and Morphology

After five summers' work in the field, Clarke began to show results, and his first three papers in a scientific journal appeared in 1882 during his professorship at Smith. They have to do with a new species of the living mollusc Gundlachia, and with rare specimens of Crustacea—phyllocarids and barnacles—from the New York Devonian. Arthropods were, therefore, his first love among fossils, and they continued all through his life to have for him a dominating interest. In these early papers he bursts upon the scientific world, as it were, as a full-fledged descriptive paleontologist, since they show nothing of the beginner; even then he wrote in a clear and direct fashion which, however, as yet displayed none of the phrase-making, quaintness, love of strange words, and power of embellishment so characteristic of his later writings.

With Clarke's appointment to the New York Survey, his career as a paleontologist and stratigrapher was assured, and from his head and hand there came a continuous stream of the best kind of geologic publications, the great bulk of which have to do with the Devonian and were issued by his native state.

His complete bibliography contains 452 titles, and of these about 300 are of a paleontologic or geologic nature, exclusive of the 76 reviews in the same fields. Together his printed books and papers cover over 10,000 pages, in which he is godfather to 135 new genera and at least 870 new forms; how many species of fossils he studied will probably never be tabulated. At least 3 genera and 42 species are named after him.

Clarke was born on Devonian rocks, and they ever remained the magnet of his endeavors. "The work of a geologist is pre-eminently what his environment makes it." The strongest pulls were those exerted by New York and southeastern Quebec, but he was also attracted by the Devonian of South America, Germany, and Maryland. Probably three-fourths or more of his total output has to do with the paleontology, stratigraphy, and mapping of this period. He long ago became one of the two greatest world authorities on Devonian, the other being Emanuel Kayser of Marburg and Munich, who has also passed away since this memoir was written. Clarke's study of the Upper Devonian faunas of Iberg, Germany, was written at Göttingen while a student of Von Koenen, and it was at this time that he became acquainted with Kayser. His loyalty to these two teachers of his is shown in his contributions to their Festschriften: "Evidences of a Coblenzian Invasion in the Devonic of eastern North America" to that of Von Koenen in 1907, and to that of Kayser in 1915, "Conceptions regarding the American Devonic."

BRACHIOPODA

Shortly after Clarke got to Albany (1886), he became deeply interested in some work that Beecher was then doing. It appears that in 1878-1879 C. D. Walcott made extensive collections for the New York State Survey at the famous Silurian locality in Waldron, Indiana, sending Hall about seven tons of fossils and slabs. In washing this material Beecher saved all the freed dirt and so got together a large quantity of minute specimens. He and Clarke, working by lamplight night after night during the years 1886-1887, picked out of these washings about 50,000 baby brachiopods, with lengths ranging from less than 1 up to 5 millimeters. Finally, they culled out 15,000 good

examples which they separated into twenty species, each abundantly represented by specimens. Arranging these in growth or ontogenetic series, they then worked out the appearance of the specific characters. Their results are set forth in "The Development of some Silurian Brachiopoda" (1889). This was probably the first ontogenetic work on fossil brachiopods, and while of an elementary nature, it prepared both authors for a better appreciation of their later studies on this class of organisms. Probably the most striking result was that the inceptive shells, of whatever species, were smooth, only slightly convex, and had a subcircular outline. This led Beecher later on to the discovery that all brachiopods start in life with this kind of shell, which he called the protegulum, and since the nearest fossil mature forms are of the genus Paterina (Iphidea), he called the protegulum the Paterina growth-stage.

When Clarke had completed the work on the Devonian Crustacea in 1888 (see page 208), Hall wanted him to take up a revision of the species of the Paleozoic Brachiopoda, but the young man had in mind a greater and more philosophic scheme. Their joint planning resulted in a turning away from a study of fossil faunas to a generic one having also to do with the phylogeny and evolution of an entire class. It was in search of material for this work that Hall came to Cincinnati in the summer of 1889, and seeing my large and well identified collection of brachiopods, proposed that I come to Albany as his private assistant, and allow him the use of my private collection—an arrangement that lasted for nearly two and a half years.

Our joint labors resulted in the work entitled "An Introduction to the Study of the Genera of Paleozoic Brachiopoda," by Hall and Clarke, which appeared in 1892 and 1894. In this memoir of 760 pages and about 90 plates, better known as "Paleontology of New York, Volume VIII," 180 old genera are redefined and 58 new ones added, while as a by-product came the description of 57 new species. While the books were in press, the study was extended to a synoptic revision of all the brachiopod genera, living and extinct, along with a summation of the known anatomy of the soft parts in the living species,

and their bathymetric and geographic distribution; it appeared in two octavo volumes under the title "An Introduction to the Study of the Brachiopoda, intended as a Hand-book for the Use of Students," the two parts being issued in 1892 and 1894, respectively. This reworking showed that the class had at least 325 genera and subgenera worthy of recognition. The four books, treating of a group of organisms that has lived since at least the beginning of Cambrian time, form the most comprehensive study ever made of a class of invertebrate fossils, and represent one of Clarke's highest attainments in morphologic and phylogenetic work. Due largely to their stimulus, researches on these organisms throughout the world have resulted in the recognition of almost 700 genera of brachiopods.

CRUSTACEA

When Clarke began his official work under Hall in 1886, he was set to work on his pets, the Crustacea. This study resulted in the memoir usually known as "Devonian Crustacea" (Paleontology of New York, Volume VII, 1888, by Hall and Clarke). In this volume, which included much new material that Clarke had collected in his earlier years and had brought with him from his home, are described 144 species (50 new) in 28 genera (9 new). Of these, 127 species are of the Devonian, 83 forms in 10 genera being of Trilobita. Among them are highly ornate and gigantic forms, the last of a rapidly declining stock. The xiphosurans are represented by 1 species, the eurypterids by 3, phyllocarids by 26 species in 8 genera, decapods by 1, phyllopods by 2, and cirripeds by 11 in 4 genera. Of this volume Clarke said in 1921:

> This book on the Crustacea was a substantial descriptive work which has served well, but it attempted nothing serious in the way of classification; it did, however, establish some interesting facts in development or ontogeny.

SPONGES

Finally came what may be regarded as the concluding volume in the series known as "Paleontology of New York." This was "A Memoir on the Paleozoic Reticulate Sponges constitut-

ing the Family Dictyospongidæ," by Hall and Clarke (1898-1899). Here are described and figured all the late Devonian and Mississippian forms of "glass sponges," of which there are 128 species. It is mainly in New York that the great fossil "sponge plantations" occur. Of new subfamilies there are 7, of new genera 22, and of new species 75, and the whole forms the most elaborated study ever made of Paleozoic sponges.

EURYPTERIDA

The most detailed morphologic, ontogenetic, and phylogenetic study of fossils made by Clarke is the joint memoir with Ruedemann on the eurypterids of New York (1912). It is only locally that these fossils are common and yet thousands of specimens were assembled, probably more than had ever before been brought together, with the result that North America was seen to have 62 species (26 are new), and 46 of these occur in New York. No locality in America ever yielded more material of this interesting group than that in the Silurian (Bertie) at Buffalo. They ranged from the Upper Cambrian into the early Permian, and were marine animals into the Silurian. Then they became euryhaline or able to live in both salt and brackish water, and finally throughout the Devonian and Carboniferous they were small and scarce animals that lived wholly in fresh water. "Some were best adapted to swimming, others to crawling and many to finding their food by grubbing in the mud."

Clarke and Ruedemann had specimens as small as 2 millimeters in length, and the ontogeny of the Eurypterida was shown to agree with that in *Limulus*. Neither line had any direct relationship with the trilobites, and their origin probably goes back to the Proterozoic, the trilobite-crustacean and arachnid lines doubtless having independent origins in chætopod annelids. The order Eurypterida was here divided into Eurypteridæ and Pterygotidæ.

Paleontology and Stratigraphy

SILURIAN-DEVONIAN BOUNDARY

While Clarke was a student in Germany, a lively discussion was going on, centering about Professor Kayser, who had in

1878, 1881, and 1884 raised the question as to the actual boundary between the Silurian and Devonian. The founder of these terms, Murchison, had established neither a top for the one nor a base for the other. In 1889 Clarke spoke to me about the "Hercynfrage," saying that he was writing an account of it, and that, as he saw it, the Lower Helderberg was the base of the Devonian in America. But how to get this paper by our watchful chief, with his quite different views? Long afterward (1915) Clarke wrote:

> Hall was so absolutely hostile to the suggested interpretation that in order to even secure publication for this array of evidence, it became necessary to change a possible argument into a neutral statement of facts, and all conclusions into queries.

Hall published the paper, however, in 1889 and I was converted to Clarke's view, namely, that the lower Pentamerus limestone (= Coeymans), and not the Oriskany, as had long been held by Hall and Verneuil, was the base of the Devonian system of rocks. Clarke returned to the discussion in 1891 and 1894 (review of Tschernyschew's "Fauna d. unteren Devon," 1893), holding "that the fauna of the Helderberg division is not Silurian is demonstrated." It was not until 1900 that I got a chance to take a stand on the question in print, and my paper in the *Bulletin* of the Geological Society of America led to another controversy, with Clarke and Schuchert on one side, and Henry Shaler Williams on the other, and it was as a result of this discussion that the two former went to Gaspé, as will be seen further on. The storm clouds have, however, long since blown away, and the Coeymans is accepted as the base of the Devonian on this side of the water. The boundary in continental Europe is now also adjusted on this basis, but in Great Britain, where the Devonian system started, the older geologists are still trying to adjust themselves to the views of the rising generation, namely, that the Ludlow closes the Silurian, and that what is above is Devonian.

ORISKANY FAUNA

In 1892 Beecher found on Becraft Mountain, near Hudson, New York, a new locality for the Oriskany fauna, and one

far more varied than any heretofore known in that State. This discovery Clarke elaborated, and published his conclusions in 1900 in the memoir "The Oriskany Fauna of Becraft Mountain, Columbia County, N. Y." Here are described 113 species, 31 being new. About one-fourth of the fauna is from the Helderbergian below, and ten forms go into the Middle Devonian. The faunal characteristics of the Helderbergian are also analyzed, with the result that everything from the base of the Coeymans up is definitely referred to the Devonian, while a review of the Manlius biota shows that it is still clearly of the Silurian.

GUELPH FAUNA

Until 1903, it was not known to what extent the Guelph faunas occurred in the Lockport limestone of New York. In that year a great deal of material, brought together by several collectors, was elaborated in the memoir by Clarke and Ruedemann entitled "Guelph Fauna in the State of New York." Here it is shown that the Lockport contains two distinct faunas, an older one, the outgrowth of the Rochester below, and the Guelph, which pulsates two or three times from elsewhere into the upper part of the Lockport. This recurrent Guelph fauna has 71 species in New York, and of these the memoir describes 14 as new. It is a fauna composed dominantly of thick-shelled molluscs and some brachiopods, which lived in a shallowing and vanishing, warm and dolomite-making sea, surcharged with salts. The authors trace the extent of this seaway and conclude that the fauna is of northern origin.

DEVONIAN OF GASPÉ

As was said above, in 1900 I had become involved in a controversy with Williams as to the lower boundary of the Devonian, while Clarke a few years earlier had assembled Oriskanian fossils from beds in northern Maine that were said to be Silurian by Williams. Both of us had read the work of Logan and Billings ("Geology of Canada," 1863) on the very long "Siluro-Devonian" section of Gaspé Peninsula in Quebec, where the Silurian was said to pass unbroken into the Devon-

ian. We therefore arranged to see this wonderful record during that summer. Together we first visited the coast of Arisaig, Nova Scotia, then that of Dalhousie, New Brunswick, where Clarke became interested, and this interest grew as we went on to Percé in Quebec, and finally to Gaspé. We then agreed to divide the work between us, Clarke to take the Oriskany equivalents, and I the Helderbergian and Arisaig faunas. In the end, however, Clarke worked out the whole of this Devonian, since there is no Silurian involved in the Gaspé section, while my students and I described the Arisaig Silurian sequence and its faunas.

On this trip of 1900, Clarke told me that he had found in Gæspé what he had long been looking for—a land of quaintness that reminded him of Scotland and the Old Red, both lands of red sandstones and of fish, fossil and recent, reawakening in him memories of his boyhood days and his reading of Hugh Miller's popular books. To this part of the Maritime Provinces, therefore, Clarke returned nearly every summer, and became the high priest of its geology. From 1900 on, he published at least fifteen papers and books on the paleontology and geology of the region, preëminent among which stands his magnum opus, "Early Devonic History of New York and Eastern North America" (1908-1909). These two quarto volumes, comprising upward of 600 pages and 100 plates, treat of the Lower Devonian stratigraphy and faunas of Quebec, New Brunswick, and Maine, fitted into the older background of the equivalent formations of New York and the Appalachian geosyncline. In this faunal study he was concerned with more than 700 species and described about 450, of which 160 were new. We do not see here that degree of paleontologic refinement shown in his study of the Naples fauna, but the stratigraphy and geologic structure of eastern Canada are far more difficult than those of central New York, and, moreover, he was dealing with Lower Devonian faunas and intricate seaways throughout a spread of about 1,500 miles. And even this was not all, for the faunas were also seen against the background of those of western Europe.

Clarke concluded that during Helderbergian and Oriskanian times the Gaspé seas were generating faunal centers that dispersed their life southwestward through the St. Lawrence trough, but that in Hamilton time the migration was reversed.

The Gaspé sequence of strata, as defined by Logan, has 2,000 feet of limestones, divided into eight divisions, and followed by about 7,000 feet of sandstones. The two basal divisions have come to be known as the St. Alban limestone (160 feet thick with a fauna of 51 species), which is followed by the poorly fossiliferous divisions 3 to 6, now called the Cape Bon Ami limestone (1,050 feet, with 11 species), and then by divisions 7 and 8 = Grande Greve limestone (800 feet with 150 forms). The St. Alban series correlates with the Coeymans and New Scotland of New York, while the Grande Greve is mainly Oriskany with some Onondaga. The overlying Gaspé sandstones have, about 1,000 feet above their base, an unmistakable Hamilton fauna (having 48 species of which 14 are new), but higher the fossils are scattering land plants and fishes that do not as yet give the exact age of the higher strata of this thick sandstone sequence, other than that they are Upper Devonian. Then followed intense mountain making—the Acadian Revolution which was succeeded by the accumulation of the Bonaventure land conglomerates to depths ranging up to 1,500 feet in the structural valleys now occupied by the Bay de Chaleur.

All in all, "Early Devonic History of New York and Eastern North America" is not only Clarke's master work, but also one of the most comprehensive faunal studies that we have in America, and shows Clarke to have been among the world's best stratigraphers.

NAPLES FAUNA

As Clarke was intending to go to Germany for a doctorate after he should be graduated from Amherst, he began in the year of his graduation (1877) thinking about subject matter for his dissertation. The Portage group had been thought to be very poor in fossils, but as he and D. D. Luther had found many of them in strata of this age, Clarke concluded to take

up a detailed study of the group and its contained faunules. As we have seen earlier in this paper, his plans for a degree in course in Germany did not come to fruition, and it was not until 1898 that the finished results of his study on the Naples fauna began to appear, and part 2 of the final report was delayed until 1904. In the meantime he had written sixteen other papers on this stratigraphic unit and its faunules. One of these has a great deal of stratal detail not in the two final papers, and this should be borne in mind by the student; it is entitled "The Stratigraphic and Faunal Relations of the Oneonta Sandstones and Shales, the Ithaca and the Portage Groups in Central New York" (1897). In Clarke's "Naples Fauna" we have one of his two best memoirs on faunal and stratigraphic studies, and it is one of the finest examples that a worker in similar fields can set before himself as an ideal to be attained.

The Naples fauna is characterized by *Manticoceras intumescens,* which occurs throughout the 600 feet of strata situated near the base of the Upper Devonian. It is a part of the Portage group, and occurs in two facies—black shales and sandstones—which alternate with one another several times. In eastern New York lay land, while to the west was deepest water, and the typical Naples fauna becomes disentangled from the shore faunas at the meridian of Cayuga, but fails to be present at the west end of the State. It is a distinct biota occurring only in the Genesee province of New York, while farther east is first the Ithaca and then the Oneonta province. The life is peculiar in that it is chiefly of the floating and swimming types, in the main bivalves (cardioconchs and Lunulicardia), cephalopods (mainly goniatites), and gastropods and pteropods. In all there are 153 species, as follows: pelecypods, 72 forms (52 new); cephalopods, 37 (25 new); gastropods, 26 (14 new); brachiopods, 7; and scattering, 11. The work yielded 37 new genera. Clarke says that the warm-water element, the Styliolina faunules, came in from the south, while the main part, with the goniatites, came in from the northwest in a colder water that "laved the northern and eastern shores" of the Appalachian gulf; and that "the proximal path of its

migration lies buried beneath Lake Erie." Finally, that at least the goniatite elements of the Naples fauna had their origin in Timan (arctic Siberia), spread thence down the Cordilleran geosyncline into the Genesee province and from there eastward to Europe. That there is this direct European linkage with the "intumescens fauna" is seen in 18 species in common with the Genesee province, along with at least 19 others having close relationships.

ZONES

Clarke was careful in his studies of faunas to note their differences and to attach definite values to them. For instance, a hemera (Buckman) is the time duration of a guide fossil like *Manticoceras intumescens,* while a zoehemera (Clarke 1898, "Naples Fauna") specifies the duration of a particular faunal province like that of the Naples fauna of western New York. A prenuncial fauna (Clarke 1898) is a heralding assemblage of a later, more fully developed fauna, as the Styliolina assemblage in the Genesee is prenuncial to the Naples one. The meaning of sequential faunas (Clarke 1898) is self-evident. On the other hand, an indigene fauna (Clarke 1902) is "one which, taking possession of the marine province at an early date, held the ground (subject to variations in its species combination) for a long period, during which may have occurred various minor invasions. . . . Every indigene fauna is alien in its inception."

EVIDENCE OF ICE IN UPPER DEVONIAN TIME

As long ago as 1843, Hall found in the Portage formation of New York grooved and striated surfaces on sandstones that in their courses are "as unbroken as in the glacial or alluvial scratches upon the surface of the present rocky strata." He could not explain these and other surface characters like "mud flows" and *"Fucoides graphica"* (regarded as seaweeds), and they have puzzled all who have seen them. Clarke had noted these markings since his college days, but not until he wrote "Strand and Undertow Markings" in 1918 (1917) did he attempt to explain them. He then said that the "mud flows" were

due to the rilling of water running down a flattened beach slope, and that some of the striations were made by floating algae dragging over the bottom, but that most of the groovings and striations were due to moving shore ice, and that the *Fucoides graphica* were the filled holes made by ice crystals growing in the bottom muds through the formation of ground-ice. "There is then reason for inferring that the late Devonian was a period of cold . . . and may well have created conditions . . . which would give plenty of means for channeling the Devonian strands of New York, by the movement of land ice toward the sea or by the landward thrust of the sea ice back from the water." *Fucoides graphica,* however, is now regarded as the burrow of a marine annelid (see Amer. Jour. Sci. (5), vol. 14, 1927, p. 159).

In the Portage at Ithaca, H. S. Williams was attracted in 1881 to "channel fillings," which channels, he explained, "were caused by the scratching of icebergs on the shoals represented by the interbedding shales." He, like Hall before him, noted that these channels and groovings strike east and west. These are interesting explanations, and, if true, must find corroborative evidence elsewhere. In "L'Île Percée" (1923), Clarke restates the evidence advanced several times before that beneath the fish beds of Upper Devonian age at the Hugh Miller Cliffs in Quebec, many of the boulders "are scored and nicked, polished and glazed, and some show without doubt the scratches and striations which could hardly have been made in any other way than by a movement on each other within a sheet of land ice. . . . Here is, then, the evidence of land ice over Gaspé at a time following immediately upon the great continental elevations which took place toward the close of the Middle Devonian."

DEVONIAN-CARBONIFEROUS BOUNDARY

Regarding the upper boundary of the Devonian, Clarke expresses himself fully in "Construction of the Olean Rock Section" (1903). The Carboniferous fossils, he says, appear at the base of the Cattaraugus in the Wolf Creek conglomerate, and it is in fossils that we have "the support of the most direct

evidence." Disconformities in horizontal strata may be more apparent than real, since in the Upper Devonian "sand reefs constantly display indications of deep decapitation due to shifting of bars and change of direction of currents, or a modification by heavy tidal flow on a shelving coast. Unconformities thus frequently exist which are no indication of unrecorded time." This short paper states clearly what should be the criteria in drawing boundary planes "in sediments characterized by the uniformity of their succession."

THE NEW YORK DEVONIAN AS A WORLD STANDARD

Finally, what the New York Devonian means in the setting of this period throughout the Americas and Europe, Clarke presents in a paper that he wrote as a tribute to the great *Devonkenner* of Europe, Professor Emanuel Kayser, on his seventieth birthday. The standard of reference throughout the world, he here shows, should be the Devonian of the State of New York, since the latter "is very properly designated a Devonic state, for more than one-half its area is covered by the rocks of the period, and the succession of its members from base to summit comprises a record whose pages are almost intact and effectively illustrate the variant happenings of the time." "Not in Devonshire nor in the Rhineland, not in the Urals nor in Siberia, not in the Bosporus or in South Africa, not in the basin of the Amazon, of the La Plata or in the Andean Cordilleras, is the full and variant succession of Devonic events so well recorded or at least so clearly and simply presented, and perhaps so fully known, as in New York."

DEVONIAN OF SOUTH AMERICA

A few years after Clarke joined the New York State Survey, his friend, Orville A. Derby, of the Brazilian Geological Survey, who was also a New Yorker by birth, asked him to describe certain Silurian and Devonian fossils. This beginning led to eleven publications between 1890 and 1919, totaling some 600 pages and 37 plates of fossils, and describing 260 species, of which 131 are new, along with 14 new genera. Of these forms,

21 (16 new) are Silurian, all the rest being of the Lower and Middle Devonian. "Fosseis Devonianos do Paraná" (1913) brings together all that is known of the Devonian faunas in South America and Falkland. The Silurian and Devonian of the Amazon region are of one faunal province and have relationships with North America, while the Lower Devonian ones of southern Brazil, the Andes, and Falkland are of the austral province and distinctly linked with South Africa. These works show that Clarke put into order and brought up to date the whole of the Devonian faunas of South America. With them, and with his far more extensive studies of the Devonian of the Appalachian geosyncline, we are now able to understand the relationships of the Devonian of the New World to that of Europe, Asia, and Africa.

The Philosophy of Clarke the Paleontologist

The philosophy which Clarke derived from his paleontologic studies is to be found mainly in five of his papers, namely, his address to the Paleontological Society as its first president in 1910 (1911); "The Philosophy of Geology and the Order of the State," being the presidential address to the Geological Society of America in 1916 (1917); "Postbellum Reflections on the Place of Paleontology among the Sciences" (1920); "Organic Dependence and Disease," published in 1919 and 1921; and "The Age of the Earth from the Paleontological Viewpoint," read before the American Philosophical Society in 1922. His conception of what the life of the past should mean to the living human world may be summed up as follows:

> The pursuit of science does not, alas, raise a man to the practice of a higher morality and it has no power to enlarge the soul of the man whose concern therein is only the acquisition of new information.
>
> Paleontology is . . . the most far-reaching of all the sciences. In it lies the root of all truth, out of it must come the solution of the complex enigmas of human society. . . . It is the panoramic display of the life of the ages, the expression of the organic law of a hundred successive worlds.
>
> I regard as peculiarly a doctrine of paleontology that of recapitulation . . . the fact that each individual carries in himself and his develop-

ment history, the history of the race to which he belongs, however accelerated or however retarded it may be.

The problem as to how species have originated . . . does not belong to paleontology. Students, both of living organisms and of fossil ones see the engrossing fact of evolution, but see it out of different eyes; the former perhaps as one would see a vast throng gathered together to acclaim a momentous event, a great victory or a high armistice; the latter as an endless army marching by, its vanguard already out of sight in the mists of the horizon, stragglers along the way falling back or giving up in hopelessness, while the interminable procession ever emerges out of the shadow.

The mutation is a clearly recognizable entity in paleontology, is the bridge crossing from species to species, the connecting link which establishes the continuity of the chain. . . . The mutation is the departure from the one, seeking adjustment and failing, or seeking and finding it in what must be recognized from accepted standards as a distinct specific form, a different species from its parentage.

The standards of our best civilization are not those which have been derived from a clear apprehension of the paramount law, [the biologic law. On the other hand, reflecting man has] some notion of how short is the way he has traveled toward excellence and how long the road that lies ahead. . . . The physical man is an item in the scheme of life . . . but it is the most compelling fact of existence that we are that item.

[All animals in Nature are born free, but not all are] born to an equality of opportunity. . . . Equality of opportunity in paleontology and in human history would mean a world of life with the chief factors of evolution eliminated. . . . If there had been equality of opportunity through the ages of paleontologic history there would be no men in the world today.

The greatest significance of evolving life as seen throughout the geologic ages came to Clarke from his studies of the earliest phases of the parasitic or dependent conditions of life—a study of mutual organic associations that lead to commensalism, sessility, and finally to parasitism. The degenerate modes of life involve

the essential abandonment of normal direct upright living and the benefactors thereby are types of life which Nature has cast out and aside as hopeless. . . . Individual and locomotive independence then, it would seem, has been the major function and prime determining factor in the progress of life. . . . All progress in life, as reckoned in terms of man, has come through independence and through those lines of animal

life in which independence has been maintained at any cost. . . . Rescue of dependents is therefore not a part of the scheme of Nature, except through the exercise of intelligence.

The paleontologist, looking at the record of life on the earth, says to the State:

Be intelligently guided in the treatment of hereditary community parasites, defectives, congenital or confirmed misdemeanants, whatever the form of degeneration may be, by recognition of the presumption that in so far as they can not be physiologically corrected, they are abandoned types in which there lies little hope of repair.

Nature makes for the individual. . . . This truth is registered on the tablets of the race. . . . Over and over again the dominant race has started on its career as an insignificant minority struggling for its existence against an overburden of mechanical and vital obstacles, armed only with specific virtues which have little by little fought their way into the foreground, and by so doing consummated their upward purpose. . . . The majority is purely numerical, while wisdom and truth may rest with the minority. . . . The voice of the people is not the voice of God. Nature's fundamental laws can not be applied. They apply themselves; they govern without consent. . . . The law is of God, the standard of man.

The simple life . . . is the life which really endures. I mean by that it is the life which is ever entering slowly into something better, through sons and daughters born and to be born, and ever leaves a greater share behind it as simple as at the start. The starting point must always endure, the seed must remain if the fruitage is to continue. We are taught to-day the immortality of the simple. . . . For the vast majority of mankind must always be of simple mind, intent upon the simple and material things of life. . . . Of poets and prophets, of men of letters and seers of science, there must always be few and they are not likely to be understood or appreciated in their own day, though they have stood guard over the jewel casket of the race ("L'Île Percée," 1923).

Of the English yeomanry, and among the first to enter through Massachusetts Bay to humanize the wilderness of America, the ancestors of John Mason Clarke followed the path of empire westward for three centuries, until, settling at Canandaigua, they gave rise to a teacher of the plain people in Noah Turner Clarke. Never bounteously blessed with the goods of this world, but rich in lofty ideals, he raised a family of leaders,

and among them one of the greatest paleontologists of our time, a builder of museums, and a statesman of science.

Always a loyal son of New York, Clarke brought to the highest excellence the knowledge of its incomparable Devonian, and ever maintained the New York sequence of Paleozoic formations as the standard of comparison for eastern North America, and in general terms for all the world. But it would be a great mistake to think that his vision was limited to the Empire State, for his work in South America and above all in his beloved Gaspesia was no less significant than that in the land of his nativity. In Quebec also, as well as in New York, he was not only paleontologist and geologist, but preserver of scenic beauty and of wild life, recorder of pioneer work in commemorative tablets, and historian of early days.

Clarke's period of descriptive and faunal work attained its climax in 1908 to 1909, with the publication of his greatest memoir, "Early Devonic History of New York and Eastern North America," and during these protracted studies he was laying deeply the foundations of his belief as to what paleontology should mean in man's speculations concerning himself and his relation to the web of nature. Between 1908 and 1922, his philosophic period, he formulated these conclusions, the gist of which is that we are born free but not equal in ability or opportunity, that socialism and communism do not lead to the growth of the intellect, that all forms of human degeneracy should be carefully guarded by the State, and that the few seekers after knowledge, the guardians of the "jewel casket of the race," must ever be ready to sacrifice themselves for the uplift of the majority, who are leading the simple life, "the life that endures," though spiritually growing through slow accretion.

An intimate knowledge of the life of John Mason Clarke reveals that his path to eminence was hewn out with much labor, taxing to the full the many-sided training that was his from home, college, and environment. Gifted with marked intellectual powers, he used his talents to the full, steadily rising as paleontologist and stratigrapher, as museum administrator, as philoso-

pher and historian. Above all, his life stands out as one of continuous service, service to his family and State, to paleontology and Devonian lore, and to Gaspé, "Coast of the Mountain Ends."

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[1] Clarke wrote many reviews (76), most of which appeared, sometimes unsigned, in the *American Geologist* between the years 1893 and 1897, but only a few of the more important of these are included in the list below.

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS

VOLUME XII—NINTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

CHARLES SPRAGUE SARGENT

BY

WILLIAM TRELEASE

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1928

PUBLISHED BY THE NATIONAL ACADEMY OF SCIENCE
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C. S. Sargent

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CHARLES SPRAGUE SARGENT

April 24, 1841—March 22, 1927

BY WILLIAM TRELEASE

"One day," said Edward Everett Hale, "a man looked up—and saw a tree."

He was speaking of our late associate, Charles Sprague Sargent: director of the first forest census of the United States; gatherer of a great museum exposition of our trees; author of the first comprehensive treatise on our woods and their properties and uses, of a compact and workable handbook of our native trees, and of a sumptuously published *Silva of North America;* founder and editor of a journal, *Garden and Forest,* which did much to promote love of the out-of-doors; stimulator of a national forest-preserve and park system; and creator and utilizer of the greatest of tree plantations:—the foremost dendrologist of his day.

Sargent was of good and efficient English ancestry domiciled in New England for over three centuries; he was related to John Singer Sargent who in the field of art rivals Charles Sprague in that of science. He was the son of Ignatius Sargent, a successful merchant, and his life was spent on the beautiful estate Holm Lea, in Brookline, immediately adjoining his father's; within a stone's throw of the home of Francis Parkman, and in relation with many men who made Boston and its environs notable in the Victorian period.

At the age of thirty-two he married Mary Allen Robeson of Boston, a talented and forceful as well as charming woman of Sotch and Huguenot stock, also Americanized for over three centuries; and their life together covered nearly a halt century in which it is hard to dissociate the aims and activities of the one from those of the other. Of their five children, four survive both mother and father.

Professor Sargent was a tall man, a little ponderous to those who knew him in his later life, quiet, never hurried, but always

forging ahead with irresistible momentum; conservative and rather hard to swerve from his course or to retard or accelerate in it. He had the reputation of being bluff and a little over-positive; but those who knew him well saw a sympathetic kindliness as underlying any superficial reactions.

Through his long life he maintained the habits of methodical industry that underlie a successful business career, in its later years dividing his time between productive activity at his desk and constructive supervision of the plantations in which his life-effort finds embodiment. This routine was broken by many and fruitful journeys—always connected with an unceasing effort to know trees and an insatiable enjoyment of personal contact with them. Though always deliberate, he was built for a long stride, and less favored companions did not find a day's tramping with him easy. On these field trips, which in later days did not involve the hardships of pack and camp-life, he uniformly found something to praise in all except the very worst of mountain and swamp hospitality, though the best was in striking contrast with the comfort to which he was accustomed at home. His zeal as a collector and his patient work in saving the fruits of a day's collecting were remarkable. Though unaccustomed to doing what he could get somebody else to do, as Professor Gray early observed and pointed out approvingly in a letter to Hooker, he always met efficiently the need of doing for himself whatever fell to his task and he did not spare himself in doing it: and his voluminous systematic work came from his own pen.

Sargent was graduated from Harvard University in 1862, and immediately entered the army and served until the end of the Civil War in 1865 as a commissioned officer. Three years of European travel followed his mustering-out, and after four more years of study and preparation he launched into his life work as Professor of Horticulture, Curator of the Arnold Arboretum, and Director of the small but famed botanical garden of Harvard University in Cambridge. The custody of this garden remained in his hand for seven years, as an apprenticeship for greater things.

Whatever of good—and it was much—had come to him

through travel, study, and association with Asa Gray, Sargent's real education began in 1873 when he was appointed by Harvard University the Director of the Arnold Arboretum-to-be. In 1879 he was given the titular Arnold Professorship of Arboriculture, a chair which he held through the rest of his life.

No other pen can picture the productive part of his life so well as one may see it in and between the lines of an account of "the first fifty years of the Arnold Arboretum" which he contributed to the Journal of the Arnold Arboretum in 1923—an impersonal, straight-forward, historical statement with few words as to the aims and ideals of the writer, who had made the history.

The Arboretum had been nominally established in the spring of 1872 through an agreement between Harvard University and trustees created under the will of James Arnold, a New Bedford merchant who had died in 1869. By this agreement, the University set aside 125 acres of land forming part of the Bussey estate which had passed to it earlier, and received from the Arnold trustees a little over $100,000, the income from which was applicable to the development and maintenance of a plantation in which as far as practicable all of the trees, shrubs, and herbaceous plants hardy in the region were to be grown and distinctly labeled. Tree knowledge, and matters naturally, directly, and usefully connected with it, were to be taught as a function of the new establishment.

Half a century later, Sargent modestly admitted that neither the University authorities who had accepted the charge nor the man selected to carry out its provisions had then an idea of what an arboretum ought to be or what it was going to cost in time and money—and he might have added in intelligence. Could James Arnold, whose modest gift (not specifically for an arboretum) started it, or George B. Emerson, a writer on New England Trees, and John James Dixwell, an equal lover of trees, who were among Mr. Arnold's trustees and gave to the endowment this direction, see what has come of their hopes they would hardly grasp the breadth and meaning that have been given embodiment in the arboretum that Sargent visualized when the privilege and duty became his.

From the first it was evident that the income available from an endowment little exceeding $100,000 (actually stated to have been less than $3,000 a year then applicable) was not adequate to properly developing and planting even 125 acres, and a decade was consumed in fighting through to consummation the first far-reaching plan of the young director; but in 1882 the title to the property was passed to the City of Boston which gave to Harvard on nominal rental a virtually perpetual leasehold on it and not only freed it from present and future taxation but undertook to construct and maintain in it roads and paths and to protect and police it—while giving to the University all freedom in carrying out the plans for the Arboretum with which the Director's mind was constantly occupied.

Ultimately the area allotted to the Arboretum was enlarged; a wall which elicits the admiration of many and has evoked the vilification of others was built along its front; roads, paths, and drainage were brought into existence; and Sargent lived to see the saplings that he planted between 1882 and 1885 grow into stately and shapely trees—isolated so that they might attain their maximum symmetry, and elsewhere so massed that the grouped effect of each species might take its part in a landscape of beauty—all labeled with an accuracy and effectiveness that could but have given pleasure as well as satisfaction to those who over half a century ago stressed the educational necessity of this feature of such a collection.

Through the main or exclusive effort of its director, the area of the Arboretum had been doubled; its endowment before his death had been increased nearly eight-fold; necessary buildings and furnishings had been provided; and a special construction fund larger than the original endowment had been accumulated.

Even as I write, less than a year after his death, the announcement is made that the new constructions for which he planned and provided are to be begun at once, and that steps are well under way toward doubling the endowment of above three quarters of a million dollars to which he himself had raised the original fund of $103,847.57, and the scope of activity of the Arboretum is said to be in process of proportionate enlarge-

ment—not beyond what Sargent foresaw but in advance of what, taking each step firmly and surely, he lived to incorporate in its successful activities.

He lived to convert the original "worn-out farm partly covered with natural plantations of native trees nearly ruined by excessive pasturage"—but including the famed hemlock hill—into a rarely beautiful park in which over 6,500 named species or varieties of choice trees and shrubs grow as representatives of 339 genera; and an incredible number of hardy plants of this kind have been introduced meantime into American and even European cultivation through its agency. To-day in its field it stands foremost.

Though methodical planting had to be deferred for more than a decade after Sargent assumed the direction, propagation and plant introduction began very early in the history of the Arboretum. His master efforts were brought to success here confessedly largely through the skill of Jackson Dawson as a propagator and of Ernest Henry Wilson, whose explorations in China—known since the days of Fortune as the home of a vast number of the choice garden plants of the Occident—have added to our treasures from this source more than had all of his predecessors combined, and on whose collections is based one of the most important scientific publications from the Arboretum.

The first decade was not given over entirely to preparations for embodying Sargent's forming plans in the great out-of-door museum that they rapidly assumed thereafter. Midway through it, the need of timber planting on the plains and the actual forest resources of the country claimed recognition in an essential item of the forthcoming census, and Sargent was entrusted with and carried out an investigation of our forests which gave him personal contact with a larger proportion of the North American trees then known than had been the privilege of any earlier student of them. It also enabled him to prepare for the American Museum of Natural History timber-size examples of their trunks—with beautiful illustrations of their botanical characters which Mrs. Sargent made, and evoked through the Watertown

Arsenal a comparative and careful study of their physical properties. The publications on this forest census and its adjuncts were and remain far more than "government documents."

More than plans for the future was in process of incubation also during the ten years in which the Arnold Arboretum lay fallow after its establishment while its director had leisure for forestal travel and statistics-gathering. Out of it has emerged a national policy of intelligent forest conservation and utilization, of salvaging the relicts of lumbering, and of preserving for future generations samples of Nature's own great arboretum in the form of national parks. Aside from his activities in connection with the census, Sargent exercised lasting influence in these directions also as a member of the Northern Pacific Transcontinental Survey (1882-3), chairman of a New York commission on the Adirondacks and Catskills (1884) and, later, as chairman of a Committee of the National Academy of Sciences on a Federal forest policy (1896).

Synchronously with these foundations for dendrology in its broad and economic aspects and for its dissemination, it was inevitable that even a less far-sighted man to whom Asa Gray was more than a name should have seen and provided for those indispensable tools of every working botanist, a library containing what has been pictured in word and pen and those vestiges of trees that preserve their characters in the herbarium for centuries—even after the giants of which they once formed part may have yielded to the assaults of man or of the elements.

Starting from a literal zero-line in 1873, the library of the Arboretum has been brought, essentially at the director's own expense, to over 40,000 publications on woody plants; and the herbarium, tracing its beginning essentially to gatherings of the census explorations, now contains specimens of a fifth of a million kinds of woody plants—among them the prized "types" of a vast number of new species to which students will refer for centuries to come as question arises in connection with still other new forms claiming recognition.

Without these adjuncts, not even the comparatively few native trees and shrubs with which the plantation started and the limited introductions from foreign lands which quickly followed

could have been accurately labeled; and without them many hitherto unknown components of our native forest flora and the vast gatherings into the Arboretum from Asia would have remained unanswered quæries: without them a "Forest Flora of Japan" could not have been written by Sargent in 1894. They were tools provided and constantly used in his rapidly growing activity.

Corresponding to the gradual material evolution of the Arboretum, its productivity in publication—for it has met its educational responsibility chiefly in this wise and through exemplifying in the open its lesson of tree-lore—has progressed by natural steps. Very early a catalog of the woody plants then in cultivation was printed, with a catalog of those being propagated (1874). Had his life been spared a little longer, Sargent's publications apparently would have ended in a catalog of the plants now growing in the Arboretum, on which he was at work at the end. A few suggestions on tree planting followed these first lists (1876), with various notes on trees and tree planting (1878, 1886), etc. As a partial forerunner of the comprehensive census and Jesup-collection publications, a short account of the forests of central Nevada appeared in 1879.

No small credit must be accorded the journal *Garden and Forest,* which he directed through its all-too-limited existence (1888-1897) as a stimulator of popular love for nature and a mentor of good taste in planting. Perhaps Sargent's first technically "botanical" papers were published in the *Botanical Gazette*—on Vitality of the seeds of *Pinus contorta* (1880), and some additions to the sylva of North America (1886); but as materials accumulated his botanical publications became frequent, often bulky, important, and always resting on an infinity of patient attention to details.

He did not need to be shown the importance of knowing what others have done before publishing one's own views or discoveries. Following the death of Engelmann, he published (1884) a list of the publications of this fellow-explorer and master in American tree-lore—of whom he was a devoted friend; and no fitter compiler of the publications of Asa Gray (1889) was to be found. Under his direction was compiled by

Alfred Rehder the five-volume quarto Bradley Bibliography; a guide to the literature of the woody plants of the world (1911-1918), toward which Sargent had been looking long.

It was while the census work was in course of publication that the idea of a full account of our trees in dignified presentation shaped itself into plans for the *Silva of North America*—Sargent's *magnum opus.* First thought of as a possible output of the Smithsonian Institution—a project which Professor Baird, then Secretary of the Institution, is said to have favored or even to have suggested, the "Silva" seemed more hopefully undertakable through a publishing house, and it was brought out by Houghton, Mifflin & Co. Unsparing in editorial and artistic talent it stands today as the most elaborate of American botanical books: costly, but abundantly paying its way, and a rarely good investment for early purchasers.

For twenty-one years the "Silva" was in process of making. The publication of its beautiful folio volumes was distributed over the years 1891-1902, and the originally contemplated thirteen had grown to fourteen. Hardly was the last volume off the press when manuscript with excellent smaller illustrations was ready for a simplified portable handbook of comparable scope, and Sargent's *Manual of the trees of North America, exclusive of Mexico* appeared in 1905, and reached a second edition in 1922 and a corrected reprint a year before his death. He lived to see, also, Rehder's *Manual of Cultivated Trees and Shrubs,* the preparation of which he had encouraged.

Except for a few of the earliest, and for these comprehensive works, Sargent's botanical publications were closely confined to descriptive botany in the field of dendrology, as a rule each dealing with a restricted topic. Such publications were inevitable in an institution which was gathering in and trying to make known collections of living and herbarium material with increasing rapidity as the years wore on. Though *finis* had been written to the "Silva," no such word can be applied to our forest flora itself—of which the trees, only, found inclusion in the "Silva"—or to the contents of parks and gardens, and it is natural that three years after the appearance of the last volume of the "Silva," a comparable publication entitled *Trees*

and Shrubs should have started at the Arboretum under Professor Sargent's editorship. Unfortunately only two volumes of this appeared (1902-1913), but in contents and make-up it forms a worthy companion-piece to the "Silva," like which the volumes were well illustrated. The marvelous success of Wilson as a collector of worthwhile plants then necessitated the editing by Sargent of a tree-volume *Plantae Wilsonianae*, enumerating the plants collected for the Arboretum by Mr. Wilson, the publication of which extended over the years 1911-1917.

A full enumeration of Professor Sargent's publications is hardly necessary here, for in 1926 there was issued from the Arboretum a list of all "Publications of the Arnold Arboretum of Harvard University and its staff, 1874-1926, chronologically arranged under authors," in which his own titles appear; and these are listed separately by Mr. Rehder in a biographic sketch published in the eighth volume of the Journal of the Arnold Arboretum, which list is appended to the present sketch, by permission.

His very latest publication, in *Home Acres* of February, 1927, fittingly deals with the realized ideal of his life: "The greatest garden in America, the Arnold Arboretum."

Sargent's style, adapted to each purpose, was simple, concise, and finished. An appreciative tone pervades the occasional tributes to friends; Asa Gray, to whom he owed great inspiration; George Engelmann, whose master knowledge of trees he revered; Charles Edward Faxon, the superb delineator of the "Silva"; Horatio Hollis Hunnewell, with whom he maintained a life-long friendly rivalry in growing the most beautiful conifers and rhododendrons on their estates, and who gave liberally for the purposes of the Arboretum.

Sargent early acquired the conservative views of "species" prevalent in the first period of Darwinian philosophy. Our western forests never have been seen by a young man more favorably environed than he was when he visited them in company with Engelmann, following Asa Gray and Sir Joseph Hooker—the most traveled of botanists. That was before *Crataegus* in the United States seemed to comprise above a dozen species (he himself has since described over 700 as new),

and when recognized plums were few in number and satisfactorily identified when encountered; and to him, then, the segregation views of certain continental systematists seemed beneath contempt, as he once expressed himself. It was before in a more casual but still very extensive way he had begun to raise seedlings in propagating material for his Arnold Arboretum planting and, like the much maligned Jordan, had found that small as well as large dissimilarities reappear generation after generation. The end of his life found him in general almost as conservative as he was in his early prime, except in the groups on which his own special studies had centered, in which he has been considered an ultraradical, though it is not impossible that as these become better known and made more easily recognizable when keyed apart on more readily observable even though less technically taxonomic differences he may achieve recognition as conservative even here.

Early in his own career, Asa Gray, keen in seeing and selecting essentials in the characters on which plants are classified, conceived the idea of presenting the genera of our flowering plants in a series of select illustrations simplified to the essentials of type. It was then that Hooker was utilizing the talent of Bauer, the master plant delineator of his day; but the remarkable skill of this great man in line drawing was surpassed under tutelage by Professor Gray's illustrator, Isaac Sprague, whose plates for the *Genera Illustrata* still stand at the head of American plant portraits as models of all-inclusive effective simplicity.

Sargent was equally fortunate in the long continued artistic cooperation of Charles Edward Faxon, whose work while lacking the extreme simplicity of Sprague's has its expressiveness and gives to the illustrations of the "Silva," of *Trees and Shrubs* and of *Garden and Forest*, and even of the "Manual" a value scarcely inferior to that of the descriptions that they accompany.

Every teacher knows that if required to draw what he describes, a laboratory student sees things not before seen by him—and sometimes unknown to the teacher. It may be that more than one keen observation recorded in the text of the "Silva" came to due notice as the more or less blurred and

obscure features of a plant found expression in seemingly novel form when standing out sharply in clean-cut line and stipple—perhaps allowed to stand only after a more or less protracted discussion in which the gentle manner of the artist prevailed with difficulty over the positive manner of the man of affairs. Even more helpful in such matters may have been the rapid presentation of contradictory facts by his equally keen younger associate Alfred Rehder, on whom his mantle in a sense appears to have fallen. To these men, as to Wilson as a collector and Dawson as a propagator—capable almost of resurrecting a dead stick and certainly of coaxing into vigorous growth a twig found in the pocket of a shooting-jacket weeks after this had been laid aside—Sargent was unstinting in recognition. Though he spoke less freely of her, through life he knew in his wife a collaborator equal to any of those whose names are joined in every mouth with his achievements, a help-meet who was an inspiring part of himself.

Publicity was not of Sargent's seeking but he did not escape many and highly prized recognitions of his talent and accomplishment. Harvard conferred the honorary degree of Doctor of Laws on him at the age of sixty. He was the recipient of medals from the Société d'Agriculture de France (1893), the Massachusetts Horticultural Society (1910), the Garden Club of America (1920) and the American Genetic Association (1923), and of the Loder Rhododendron Cup from the Royal Horticultural Society of England (1924). The Veitch memorial medal was given him in 1896. He was elected to membership in the National Academy of Sciences in 1895; and held honorary or corresponding membership in many of the best organizations touched by the interests of his life.

In the activities which make Boston what Boston is, his sterling integrity of character, good taste, and conservative business sense necessarily enlisted his service, though in general he shrank from active participation in even professional gatherings. Among the offices that he long and serviceably held are the presidency of the somewhat patrician Massachusetts Society for the Promotion of Agriculture, which he held for a quarter-century; the vice-presidency of the Massachusetts

Horticultural Society, for half a century; and trusteeships, further, in connection with the Boston Museum of Fine Arts and the Brookline Library. He also served Brookline as a Park Commissioner.

Dear to a botanist's heart is commemoration in the names which fellow-craftsmen give to newly discovered plants. Numerous garden varieties and spontaneous varieties and species have been dedicated to him by their describers, and *Sargentia* and *Sargentodoxa* are genera named in his honor respectively by Sereno Watson, and by Rehder and Wilson, while a subgroup of *Prunus* has been called *Sargentiella* by the German dendrologist Koehne.

Wherever Professor Sargent's life touched or intersected the lives of men he will be remembered gratefully. In helpful service along many lines his life is memorable; but outstanding above all is the vision and the creation of his most serious life-effort, the Arnold Arboretum. In these grounds, which attract more lovers of the beautiful in plants than all other Boston offerings and to which students of trees come from the four quarters of the earth, might well be found a simple tablet bearing his name and that beautiful line marking the resting-place of Sir Christopher Wren in St. Paul's Catedral:

Si monumentum quaeris, circumspice.

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OF

HORACE LEMUEL WELLS
1855-1924

BY

RUSSELL H. CHITTENDEN

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1925

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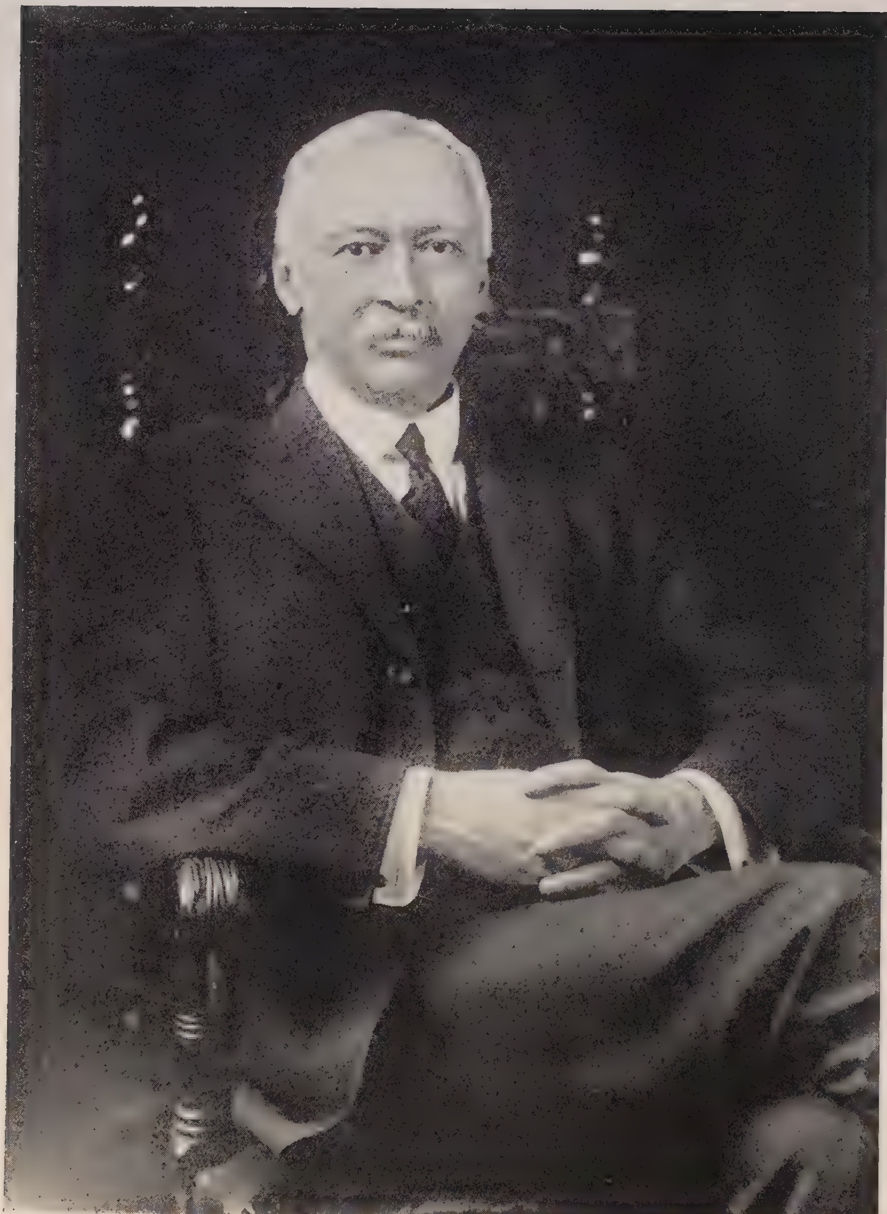

Horace L. Miles.

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Horace Lemuel Wells was born in New Britain, Connecticut, on October 5, 1855, of vigorous English ancestry long resident in New England, the first American ancestor on his father's side being Thomas Welles, the fourth Governor of Connecticut.

Governor Thomas Welles, stated to be a lineal descendant of the Essex branch of the De Welles family in England, was born in Essex County in 1598 and came out to the colonies in 1636 as private secretary to Lord Saye and Sele, who was interested in the Connecticut Patent and who in the year preceding had commissioned John Winthrop, son of Governor Winthrop of Massachusetts, to erect a fort at the mouth of the Connecticut River. When his lordship and company arrived at Saybrook he quickly became discouraged by the dreary aspect of his surroundings and the dim prospect of his golden dreams being realized and he soon returned to England, leaving his secretary to combat the difficulties and dangers of the wilderness as best he could.

Thomas Welles, nothing daunted by the new and strange conditions, proceeded up the Connecticut River with his company to Hartford. Here, in 1637, he was chosen one of the magistrates of the Colony, a position he held for a period of twenty-two years. He was the first Treasurer of the Colony under the new Constitution and in 1641 he was chosen Secretary of the Colony. In 1655 he was elected Governor and again in 1658. Thus, up to his death in 1660, Thomas Welles continually enjoyed the confidence of his fellow-citizens and served the new Colony in the highest posts within the gift of the colonists.* Horace Lemuel Wells was the eighth in direct line of descent from this eminent English colonist.

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* These statements are taken from the "History of the Welles Family in England and Normandy." By Albert Welles. New York, 1876.

Nearer the modern end of the ancestral line there was an intermingling of the Wells blood with the Sedgwicks and Websters of New England, Horace Wells, the grandfather of the subject of our memoir, having married Pamela Sedgwick of West Hartford, whose grandmother was Miriam Webster. Levi Sedgwick Wells, the father of Horace Lemuel, married Harriet Francis, whose mother was Mary Tobey, a direct descendant of Thomas Tobey of Sandwich, Cape Cod, one of the original grantees of this first settlement on the Cape in 1637. Consequently from both sides of his ancestral line Horace Lemuel Wells, if heredity and environment exercise the influence we assume on character and ability, was fully endowed with traits that contribute to the development of courage, self-reliance and industry.

The boyhood of Horace Lemuel was spent on his father's farm near New Britain, where he enjoyed the blessings of outdoor life and early acquired that love of nature that characterized his mature years. For associates and playmates he had an older brother and a younger sister, but the woods and the fields near his home had for him a special attraction and he found much to satisfy the desires of his inquisitive mind in the trees, wild flowers, fungi, and song birds that he met with in his daily rambles. During his school days, spent in the public schools of New Britain, he began to think of botany as a career, and with his analytical mind and habits of close observation he would have made undoubtedly a success in that field of scientific work. The home influences, however, tended to draw his youthful mind toward a business career, for his father, though living on a farm, was president of the local bank, public-spirited and active in many movements for the betterment of local conditions. These matters while interesting to the young lad did not appeal to his imagination sufficiently to draw him away from thoughts of a scientific career, vague though they must have been, for he had an instinctive love of the mysteries of nature and a keen desire to peer beneath the surface. Both his father and mother in their younger days had taught school, the one in the typical red school house of New England and the other in a more dignified Academy, and it is quite likely that the son had acquired from some near or remote ancestor not only his scholarly in-

stinct, but also a desire to impart to others knowledge which he had gained by careful thought and study. However that may be, it was decided that he should go to college, and having finished his course at the New Britain High School he entered the Sheffield Scientific School at Yale in September, 1874, when he was nineteen years of age.

His college life brought to him new experiences and opened up new thoughts and aspirations. Closer acquaintance with the experimental methods in chemistry and physics awakened his profound interest in these sciences and he seems to have debated whether he might not find here a special field for his future work. Further, he was attracted by the sciences of geology and mineralogy and he saw in his thoughtful way how closely chemistry was related to physical geology and to mineralogy. The applications of chemistry to these two latter sciences as well as to many other sciences and to problems in industrial life led him to magnify, perhaps unduly, the significance and importance of analytical chemistry. Again, Wells was naturally a very careful, painstaking worker, and consequently he was greatly attracted by these requirements in chemical analysis as well as by the exactness so necessary in analytical work. Further, I have heard him say that the *difficulties* in the way of exact chemical analysis always had for him a great attraction, and this fact perhaps added some weight to the other reasons which finally led him, while still in college, to choose analytical chemistry as his life work.

Wells was fortunate in his undergraduate life in having three classmates and close friends, who like himself were thoughtful, serious-minded students intent on gaining from their college experience all the advantages possible not only from class room and laboratory, but by association and discussion with professors and instructors and with each other. All four of these young men had decided on their life work early in their college course, and their many serious discussions regarding the advantages and disadvantages of a career in a given science undoubtedly helped each one in the crystallizing of his ideas into definite shape. These classmates were Joseph P. Iddings, later Professor of Petrology, University of Chicago, and a member of the National Academy; Samuel L. Penfield, later Professor of Mineralogy at Yale, also a member of

the Academy; and William T. Sedgwick, later Professor of Biology at the Massachusetts Institute of Technology. This was a rare group of young men, each destined to become a recognized leader in his chosen field of work and each no doubt contributing much during the years of close association in their undergraduate life to broaden the mental outlook of the others. It is interesting to note that of these four men, three were fitting themselves for closely allied fields of work.

At the end of his junior year, Wells received the prize for excellence in chemistry, and on his graduation in June, 1877, with the degree of Bachelor of Philosophy, he was chosen on the basis of merit to read a portion of his thesis at the Anniversary Exercises. This thesis was entitled "Determination of Titanic Acid in Iron Ores Containing Phosphoric Acid." After graduation, he spent the following year as a graduate student in the Sheffield Scientific School, carrying on advanced work in chemistry and mineralogy under Professors Samuel W. Johnson, Oscar D. Allen, and George J. Brush. During the next two years he served as an assistant chemist at the Connecticut Agricultural Experiment Station, devoting such spare time as he could obtain to furthering his advanced studies in the Sheffield Scientific School. For a short period, he was assistant chemist under Dr. Charles B. Dudley at Altoona in the laboratory of the Pennsylvania Railroad. In the latter part of 1880 he went to South Pueblo, Colorado, as chemist to the Colorado Coal and Iron Company, where he remained for four years, gaining much practical knowledge in both chemistry and metallurgy.

Wells had now reached the turning point in his career. For six years he had been occupied mainly with work in practical chemistry and metallurgy. This experience had broadened his vision and given him a clearer knowledge of the applications of chemistry to industrial operations, but he realized fully that this was not the line of work for which he was best fitted, neither did it give him opportunity for the scientific study and research he longed to carry on. The time had not been wasted. He had learned much, but he realized that the future of which he had dreamed and for which he had struggled to prepare himself was not to be attained by continuance in the path he was following. Providentially just then there came a call to

return to Yale, as instructor in analytical chemistry in the Sheffield Scientific School. This he accepted at once, and the fall of 1884 found him back in New Haven engaged in work that was in every sense congenial. Here he was associated with Professors George J. Brush and Edward S. Dana in mineralogy, and what was of almost equal importance to him, with his classmate, Samuel L. Penfield, then instructor in mineralogy. In chemistry proper he had the advantage of association with Professors Samuel W. Johnson, Oscar D. Allen, and especially with the younger men, Louis V. Pirsson and Thomas B. Osborne, both later members of the Academy. The way was now open for him to make progress in the direction where his chief interests lay, and under most satisfactory conditions. To undergraduate students he gave instruction in qualitative analysis and later in quantitative analysis, but he had ample time to carry on the research work he longed to do. His first published paper on "Gerhardtite and artificial basic cupric nitrates" appeared in the American Journal of Science in 1885, in cooperation with Samuel L. Penfield, and from that date until the year prior to his death there was a steady output of contributions giving the results of his investigations.

At the time when he began his research work, there was great activity in the field of mineralogy at New Haven. Professors E. S. Dana and George J. Brush had found a wealth of new material in the mineral deposits at Branchville, Connecticut, and it was quite natural that Wells, with his marked ability as an analyst, should be drawn into the study of these new minerals which Dana and Brush were investigating. Consequently, some of his earlier papers dealt with the composition of a number of these new minerals. With E. S. Dana he described the new mineral Beryllonite, he determined the composition of a new platinum mineral which he named Sperrylite, and with Brush and Dana he analyzed and described several manganesian phosphate minerals from the Branchville locality. He also studied and described with Dana some selenium and tellurium minerals from Honduras. He was likewise occupied for some time with a study of various basic salts, notably basic lead nitrates, and basic zinc and cadmium nitrates, the results of which were published in the American Chemical Journal. During 1889 he broadened his experience by work-

ing for one semester at the University of Munich, where he absorbed much that was useful to him in his later life.

Wells was gradually making for himself a definite and well-recognized position at Yale, and in 1888 this position was stabilized by his appointment as assistant professor, followed five years later, in 1893, by his appointment as professor of analytical chemistry and metallurgy and a member of the Governing Board of the Sheffield Scientific School. Graduate students in chemistry were beginning to turn to him for their thesis work and many important investigations were carried on jointly by Wells with these advanced students. He was not only a good teacher, training the men in exact methods of chemical research and leading them on to independence of thought and action, but in addition he was endowed with that rare quality of inspiring those with whom he came in contact with the true spirit of sound scientific research. He was a man of clear intelligence and strong will, with a whole-hearted devotion to his chosen field of work, which impressed his students and gave them a feeling of respect and admiration both for his ability and his quiet but persistent efforts for their development. He had a firmness of conviction that was not easily shaken regarding the proper course to be pursued in carrying on an investigation. No halfway methods were allowed in his laboratory. Nothing short of the greatest possible accuracy would satisfy his mind and no effort was spared to insure that exactness which the conscientious and careful analyst aims to attain. He was truly a brilliant analytical chemist.

His whole life was given to the one purpose of upbuilding the science of chemistry in the Sheffield Scientific School and no work was too arduous for him to undertake. He was more or less responsible for all the undergraduate instruction in chemistry given in the Scientific School, except in the field of organic chemistry and in the elementary courses pursued by the Freshman class, for a period of thirty-five years. He assumed the duty of general oversight and upon him rested largely the selection of the younger instructors and assistants needed to carry on the courses of instruction. But it was his plan of work that was followed and over it all he kept a careful supervision. In this, however, he did not interfere unduly

with the younger men, but left them free to pursue their own methods, so long as results were satisfactory. He also had charge of the instruction in metallurgy and assaying during a long period of years, and for this work he was admirably equipped by his earlier experience in South Pueblo. Further, he was called upon to plan the new Sheffield Chemical Laboratory, which in 1893 was erected to supplant the old laboratory in Sheffield Hall, then wholly inadequate to care for the many students applying for admission. This work he undertook with his customary enthusiasm, with the result that the new laboratory planned wholly by Professor William G. Mixter and himself was without question one of the most convenient and satisfactory laboratories of chemistry in the country at that date. All the time and thought required by this work he gave willingly and unselfishly, seeing in it merely another opportunity to aid in placing chemistry in the Sheffield Scientific School on a higher and more fruitful plane.

In 1891, Professor Wells began a series of studies on double salt formation, especially compounds of cæsium, extending over a period of thirty years, upon which his reputation as a scientific investigator largely rests. Up to the time he began this work the element cæsium was exceedingly rare, having been found only in extremely small quantities, and consequently little was known concerning its salts. Fortunately, in 1891, he obtained a large amount of the rare mineral *pollucite,* rich in cæsium, and from this he was able to obtain several kilograms of pure cæsium salts with which he began his studies on double salt formation. At first, he prepared a variety of perhalides of cæsium, salts having a beautiful crystallized form, and later he made a great variety of entirely new double salts, the crystallographic forms of many of which were studied by Professor Penfield. This work led to a systematic study of double salts containing cæsium, reinforced later by a study of the halides of rubidium and potassium, both of which elements are closely related to cæsium. His investigations were in many respects remarkable and led to an accumulation of data of great value in throwing light on double salt formation in general. His study of the trihalides was also especially important. A glance at the bibliography will give some indication of the great variety of the compounds pre-

pared and studied in this long-continued investigation of double salt formation, largely compounds of cæsium.

In 1902, he published in the American Chemical Journal a long and important article dealing with the discovery of a remarkable series of triple salts containing cæsium, notably triple thiocyanates. The discovery of these compounds opened up a new and important chapter in inorganic chemistry. Twenty-three double thiocyanates, all but one of which were new, and fourteen triple salts were prepared and studied, all well crystallized. He came to the conclusion that the triple compounds and the double salts are governed by the same laws, but the thiocyanates generally form double salts in smaller variety than the halides. These triple salts continued to occupy his attention for many years, and the last paper he published was "on a cæsium-cupric-mercuric triple chloride" in 1923.

During these thirty years or more of active research he was aided by a large number of graduate students, who looked upon the privilege of working with Wells as a golden opportunity to acquire from a master mind the method and spirit of the chemical investigator. He demanded much in the way of steady and earnest effort, scrupulous care and exactness of result, but he in turn gave much. He was intensely interested in all the men who studied under him and worked with him and their welfare and success were always close to his heart. To them he gave generously of his knowledge and experience, and he was a friend to whom they could go freely for advice and help. Wells was a man of strong likes and dislikes, with an intense love for inorganic chemistry, and for all those who showed an interest in his chosen field of work he had a strong feeling of respect and a desire to encourage and help them forward. But for the man who was not thoroughly honest and sincere in his endeavors and who was not willing to give his whole mind to his work he had an equally strong feeling of dislike and distrust. To such a one he had nothing to give.

Professor Wells was widely read in chemistry. While his chief interest naturally lay in the field of analytical and inorganic chemistry he had a deep interest in all that related to chemistry in general and his knowledge was profound and extensive. For more than twenty years he was an associate

editor of the American Journal of Science and he contributed a large number of reviews and scholarly criticisms of books and scientific articles to that journal. To facilitate the use of the best analytical methods he translated in 1897 the last German edition of Fresenius' Manual of Qualitative Analysis, and the following year he wrote and published a smaller text book dealing with analytical methods which is widely used. He likewise prepared and published a book entitled "Chemical Calculations," designed primarily to facilitate calculations in analytical chemistry. This book, together with a later textbook of "Chemical Arithmetic," has proved of great service to students of chemistry at Yale and elsewhere. In 1901, when Yale University celebrated her two hundredth anniversary, he edited two large volumes of researches from the Sheffield Chemical Laboratory published under the title of "Studies from the Chemical Laboratory of the Sheffield Scientific School."

The work Wells accomplished gained for him well-deserved recognition. Yale, in 1896, conferred upon him the honorary degree of Master of Arts. The University of Pennsylvania in 1907 gave him the honorary degree of Doctor of Science, and in 1903 he was made a member of the National Academy of Sciences. On his retirement from active service in 1923, as professor Emeritus, the President of the University in his annual report made the following statement: "The close of the college year marks the retirement to the 'Emeritus' group of Horace L. Wells, Professor of Analytical Chemistry and Metallurgy, after a long and distinguished career in the service of the (Scientific) School. He was responsible to a very large degree for the excellence of the instruction in Chemistry, which has always characterized the School's work and for the development of the work of the Department in all branches of chemical science. Actively engaged in research himself, he was indefatigable in inspiring and helping others in their investigations."

Amid all his duties in the University and his devotion to his research work, Wells found time for social intercourse with his many friends, both inside and outside the academic circle. He made friends readily, his receptiveness, his unfailing courtesy and kindliness and his intellectual honesty all combined to render him a very lovable companion. In his home

circle he was extremely fortunate. In 1896 he married Sarah Lord Griffin, of Lyme, Connecticut. They had two daughters who grew to be the close companions of their father, deeming it their greatest privilege to accompany him on his walks into the woods and fields about New Haven to study the plants and fungi which Wells delighted in. He taught the children the names of the flowers, the localities where the rarer species grew and found pleasure and relaxation in training them in habits of close observation of nature's ways. For young children their knowledge under his tutelage became quite profound and it was a delight to see them with their hands full of various specimens, the Latin names of which they rattled off as fluently as any mature botanist. Wells all through his life maintained his early interest in botany, especially the fungi. Of mushrooms he had a profound knowledge and the haunts of the edible forms he knew thoroughly. He delighted in bringing home some specially toothsome variety and having a chafing dish party with his family and friends about him.

Wells died at his home in New Haven on December 19, 1924, in his seventieth year, being survived by his wife and two daughters. One daughter, Gertrude Griffin, is married to Danford N. Barney, Jr., and the other, Evelyn Salisbury, is the wife of General Charles H. McKinstry. But he left others to mourn his loss aside from the members of his family. Friends and colleagues there are who miss his genial presence, and a large group of those who may claim intellectual inheritance, former students to whom he had transmitted, in part at least, his enthusiasm, sterling honesty, and love of truth.

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OF

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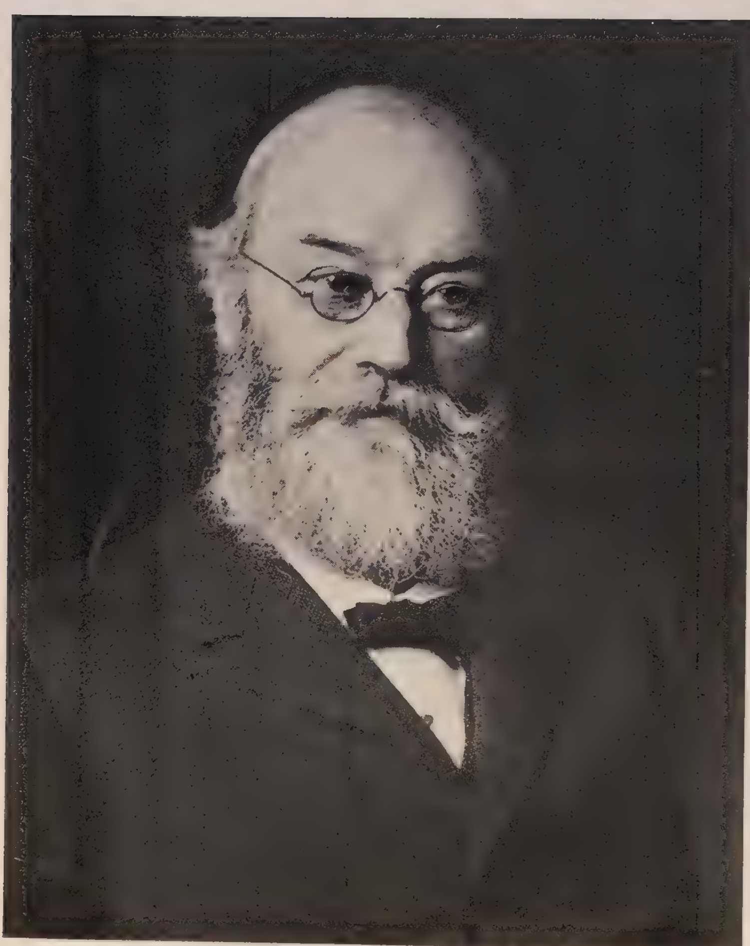

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Professor William H. Brewer was a conspicuous figure in American science, a man of broad sympathies, wide interests, public-spirited to a high degree, and, above all, a scholar and a man of research. Living in a period when science was less specialized than at present, he was drawn into many fields of endeavor, on all of which he left the impress of his strong personality and his breadth of knowledge. Filled with a love and reverence for natural science in all its branches, he was not content to limit himself to any one special field of work, but passed from one to another with a freedom and purpose that bespoke profound knowledge and a deep-seated desire to bring to the aid of the public the benefits to be derived from the applications of science. He was a typical representative of the old school of American educators, a type we shall probably never see again. For thirty-nine years he held the chair of Agriculture in the Sheffield Scientific School at Yale, but that title by no means represents the scope of his activities; still agriculture and the sciences which contribute to its development constituted the nucleus around which his thoughts and endeavors revolved.

Brewer's ancestry was chiefly Dutch and French Huguenot, and in minor degrees Danish, French, and Scotch-Irish. His father was a descendant of Adam Brouwer Berkhoven, who emigrated from Cologne in 1642 and settled in New Amsterdam (New York). The name Berkhoven may have been his surname, but more probably indicated that he came from Bercham in North Brabant in the Netherlands, for he was of Dutch and not German origin. This name, however, persisted as late as 1700, after which the name Brouwer was used, soon changed however to Brower. The oldest tide mill on Long Island, at Gowannis Creek, in Brooklyn, owned by some member of the family, was long known as Brower's Mill.

Shortly after the close of the Revolution, there was much

patriotic changing of names and William Brewer's grandfather, Nazareth Brower, who lived near Poughkeepsie, changed his name to Brewer. Of the fifteen children of Nazareth only two, however, followed the example of their father and adopted the new name. Henry, William Brewer's father, born in 1804, took the new form, Brewer, and retained it throughout his life.

William Brewer's mother was Rebecca Du Bois, born in Poughkeepsie in 1800, and married to Henry Brewer in 1827. She was a descendant of Louis Du Bois, who was born in 1626 at Wicres, in La Bassée near Lille, in French Flanders, now the old province of Artois. He was a Huguenot and on account of religious persecution went to Mannheim, Germany, where he married in 1655 Catharine Blanchen, also a Huguenot refugee. With their two children they came to America in the summer of 1660, settling in the Huguenot colony in Ulster County, New York, first at what is now Kingston, moving in 1662 to New Village, or Hurley. The following year this village was destroyed by the Indians, and the wife of Louis and her three children were carried off and held captive for three months, when they were rescued by Dutch troops sent from New York. In 1666 Louis Du Bois with some other settlers purchased from the Indians land on the Wallkill River and established the town of New Paltz, where he died in 1695.

William Henry Brewer was born on September 14, 1828, at Poughkeepsie, New York. Shortly after, his father bought a farm at Enfield, six miles from the village of Ithaca and settled the family there in the spring of 1830. On this farm young Brewer spent his childhood, going to the district school in the earlier years and later to the nearby Ithaca Academy, which he attended for four consecutive winters until the spring of 1848. During the summer months he worked on the farm with his father, acquiring a live interest in all that pertained to agriculture. Evidently the father was a progressive man, keenly appreciative of the advantages to the farmer of a knowledge of the sciences relating to agriculture. Consequently he listened sympathetically to the expressed wish of William, now twenty years of age, that he be allowed to go to New Haven and enter

the "School of Applied Chemistry," under Professors John Pitkin Norton and Benjamin Silliman, Jr. Permission being granted, he started for New Haven in the fall of 1848, taking his first journey in a public conveyance.

The "School of Applied Chemistry" started at Yale in 1847, the beginning of the Sheffield Scientific School, was dominated largely by John Pitkin Norton, who from his training abroad had gained a wide knowledge of chemistry, especially in its applications to agriculture. Brewer had read accounts of some of Norton's researches, and it was this that had inspired the wish to study with the man who knew so much about scientific agriculture. Possessed of a high degree of intelligence and endowed with unbounded enthusiasm, Brewer entered on his work in the Analytical Laboratory with a determination to acquire all there was to know as speedily as possible. But after a short experience he began to realize there was much to learn; chemistry, mineralogy, geology, botany, German, French all aroused his interest, and we find from his diary that he soon understood he must take more time than the year originally planned. He records the great satisfaction his father's permission to spend two years in study in New Haven gave him. These years were full of profit, not only in the accumulation of a fund of useful knowledge and experience, but the country lad with limited vision who left the farm for the first time in 1848 was rapidily growing into a man of broad outlook, with a true appreciation of relative values, who realized that the natural sciences were destined in the near future to occupy a position of supreme importance in the development of practical agriculture and in the improvement of the industrial life of the country. Among his more intimate associates in the chemical laboratory were Samuel W. Johnson and George J. Brush, with whom he was destined later to coöperate for many years in the upbuilding of the Sheffield Scientific School.

When he returned home in the summer of 1850 Brewer was in some doubt as to his future. The promotion of agriculture through the sciences had been constantly in his mind and he had been studying with that object always before him, but the two years at Yale in close association with such men as Pro-

fessors Silliman, Senior and Junior, and John P. Norton, had given him new thoughts and aspirations. Life on the farm appeared less attractive, and it is clear from his diary that he was gradually turning towards teaching as a profession. He saw that the country had need of schools or colleges of agriculture and he thought possibly here might lie his opportunity. In the meantime he worked on the farm during the summer and fall of 1850, but in December of that year he entered the Ithaca Academy as a teacher of chemistry. There he had a small laboratory and much time was spent in arranging such illustrated lectures on chemistry as his limited apparatus would permit. He was far from satisfied with his surroundings, however, and longed for an opportunity to do something more directly connected with agriculture. He was thinking constantly of a school of agriculture, and he relates in his diary of that date a long conversation with a Swiss medical student regarding the agricultural schools of Switzerland, how they were conducted, the character of the curriculum, and the success of the students after completing their studies.

Greatly to his delight, in the spring of 1851 he was invited to take charge of the Oakwood Agricultural Institute at Lancaster, in Erie County, New York. This he accepted with alacrity, thinking that here he might be able to put in operation some of the plans he had outlined in his mind for a school of agriculture, but when he took up the work at Lancaster he found conditions so impossible, students so few in number, and equipment so poor and scanty, that no satisfactory progress could be hoped for. It was a grievous disappointment, but he remained there through the year in accordance with the agreement, doing the best he could under the adverse circumstances. As he wrote in his diary, however, there was no chance to do any satisfactory work, but perhaps it was good discipline for him. At the close of the school year, in April, he returned to Ithaca, doing a little teaching, but devoting most of his time to studying botany and French in preparation for the forthcoming examinations at New Haven, when he hoped to take the bachelor's degree. Yale had decided to grant the degree of Bachelor of Philosophy to students in the "School of Applied

Chemistry" who met the specified requirements, and on Commencement Day, 1852, six men received the degree, the first class to graduate from what is now the Sheffield Scientific School. Among these six were William P. Blake, George J. Brush, and William H. Brewer.

An interesting sidelight is thrown upon the character of the examinations in Brewer's diary of that date. The examinations, he wrote, "were much more rigorous than any of us had anticipated. In chemistry, each was taken in a different manner and no one as he had expected. The examinations in this science continued about an hour each, the questions by Benjamin Silliman, Jr., in the presence of Professor John Pitkin Norton and Dr. J. Lawrence Smith. In geology the questions were by Professor Silliman, Sr. When we understood that he was to examine us in that science there was rejoicing, as none of us supposed that he would be minute, but in this we were decidedly mistaken. He was by no means as inclined to let us go with mere general principles as we had anticipated, but was minute and exact on many points where we least expected it. The botany and mineralogy were by Professor James D. Dana and were as thorough as the rest. The French was by M. Legendre."

Having obtained his degree, thus completing satisfactorily the first chapter in his career, he was uncertain what step to take next. Hampered as he was by the need to secure sufficient money for his expenses, there were necessary limitations in his choice. Above all he wished to go abroad and study with some of the renowned European chemists, but that was at the time not possible. With his ardent love of the open air and all that pertained thereto, he had found great pleasure and satisfaction in exploring the region about New Haven and that section of New York State where he had lived. He had wandered far afield in the study of flora and minerals. The mountains and the valleys were for him an inspiration and a delight, and nothing gave him greater pleasure than to seek and study the plants and minerals in hidden and difficultly accessible places, whether on the mountain top or in the deep gorges. All through his life this love of untrammeled nature showed itself, and when

later on events so shaped themselves that he could travel far into the wild and even into the unknown, his cup of happiness was filled to overflowing. In the meantime he had to support himself, and he applied for the position of geologist and mineralogist on the Gunnison Expedition, an exploring expedition beyond the Rocky Mountains. Fortunately, on account of a delay of the mails, he lost the appointment and so by the merest chance was prevented from joining that party, all but four of whom were murdered by the Indians near Salt Lake.

Again he took up teaching in an academy, this time at Ovid, New York, where he gave the instruction in physiology, natural philosophy, and agricultural chemistry. This was a much more satisfactory position than he had had previously and for three years he remained there, until August, 1855, reasonably contented, but always looking forward to the time when he could go abroad and increase his knowledge of the world and of the sciences in which he was so deeply interested. His success at Ovid Academy was very marked and tended to strengthen his faith in his ability as a teacher. He also gave many courses of illustrated lectures in near-by towns on chemistry and natural philosophy, which helped to swell his income and thus brought nearer the time when his cherished hope could be realized. All his spare time, however, was spent in botanical work, collecting and studying the plants, and especially the mosses of that section of New York State. He also had some correspondence with Agassiz, and he undertook to collect for him the fishes of that region to aid Agassiz in his great work on the fishes of the United States. Finally, however, he found himself in a position to carry out his long delayed plan of going to Germany, and in September, 1855, he sailed for Hamburg, going by sailing vessel for economy's sake, saving thereby seventy or eighty dollars; a voyage of forty-one days.

For two years he was abroad, spending the first year at Heidelberg, mainly with Bunsen, while the larger portion of the second year he was at Munich working under the renowned Liebig. Chemistry naturally occupied the greater portion of his time, but geology, mineralogy, and especially botany were given some attention, particularly during the vacation periods when

he made many excursions throughout Germany and Switzerland collecting and studying botanical specimens and minerals. In some of these excursions he covered much territory. Thus, in the summer of 1856, he walked six hundred miles through Switzerland, botanizing in many localities and collecting a wealth of botanical material. Again, he took a month's walking trip, geologizing in the Tyrol Mountains. He visited the great Hohenheim Agricultural School, near Stuttgart, at that time perhaps the most noted school of its kind in the world, and made a careful study of the methods of instruction employed there. Later, while in France, he spent some time at the École Imperiale d'Agriculture at Grignon, the most complete agricultural school of that country, and while in England prior to sailing for home he visited many of the more noted English farms. His stay in France was short, but he found time to go with the Botanical Society of France on an excursion of about two weeks in southern France, where he saw much that was new to him. In Paris he was able to attend the lectures of the distinguished chemist Chevreul, during his two months' stay in that city.

Thus ended the two years of European study, but the mere recital of these facts gives only a faint suggestion of all that he absorbed during this eventful period. As one reads the many letters he wrote home during these two years, it is easy to perceive that Brewer quickly caught the spirit of the great masters with whom he was studying, and that they inspired in him an enthusiasm for better means and methods of instruction and research in the sciences to which he was devoted. Under Liebig, particularly, he gained broader views of the many ways in which chemistry could aid agriculture, both directly and indirectly. He evidently looked on Bunsen as the more profound and brilliant chemist, but Liebig gave him much to think about in matters pertaining to the life processes of animals and plants, the cultivation of the soil and the broader aspects of agriculture as a science and an art.

There is no evidence in Brewer's letters of that date that he contemplated taking up chemistry as a profession. Chemistry with him was a means to an end, that end being agricul-

ture, but he was so in love with the natural sciences that he worked at them all with almost equal zeal. Even in these earlier years he had an encyclopedic mind, and it simply was not in him to confine his intellectual activities to any one field of work. His temperament was such that he took a keen and vivid interest in everything about him, and he was rapidly accumulating a fund of information and original knowledge on many subjects, destined to be of great help to him in later life. His letters during this European experience were filled with observations of all kinds, not only on scientific matters but on social customs, habits of life of the people, political and economic problems, etc. Nothing was too insignificant to be noted and remarked upon, nothing escaped his eyes, and as a result, when Brewer returned to America, he came back equipped with an amount of special and general knowledge possessed by few men of his age.

After his return from abroad he accepted the professorship of natural sciences at Washington College, Pennsylvania, now Washington and Jefferson College, but in 1860 an opportunity presented itself which he was quick to seize. In that year the State Legislature of California authorized a geological survey of the State calling for, among other things, a full and scientific description of its botanical productions. Professor J. D. Whitney was the geologist in charge and he offered to Brewer the position of first assistant on this survey. Consequently, on October 22, 1860, Brewer sailed for California with all the baggage and instruments for the survey, by way of the Isthmus of Panama, reaching San Francisco November 15. He was astonished at the size and appearance of the city, then only about ten years old, not expecting to see such large streets and fine buildings as met his eye. On the first of December the party was at Los Angeles, then a city of about 3,500 inhabitants, and on the plain about twenty miles from the sea and fifteen miles from the mountains they made their first camp and started the work of the survey.

For four years Brewer had charge of the field parties during the summer, spending the winters in San Francisco working up his notes. This survey under Whitney was thorough and

comprehensive. They climbed and named many of the higher mountains, mapped the country and reported on the geology, botany, mines, etc., of the State. While Brewer had general oversight of all the field work his special duty was the study of the flora of the State. The botanical collections were made entirely under his direction and mostly by him. The total number of species collected was about two thousand, and when it is remembered that these plants were growing spontaneously over an area of about 160,000 square miles, it is clear that Brewer must have become thoroughly acquainted with the geography of the State of California. In his Journal, under date of December 19, 1862, he wrote, "The field work for the year has closed. I have been adding up my perigrinations in this State since I arrived twenty-five months ago, and the following are the figures: mule-back, 3,981 miles; on foot, 2,067 miles; public conveyance, 3,216 miles; a total of 9,264 miles. This has been over an area 625 miles long in extreme length, and has been nearly all in the coast ranges. Probably no man living has so extensive a knowledge of the coast ranges of this State from personal observation as I have, but I have seen very little of the grand features of the Sierras." He had, however, two years more of exploring in California before his work there was finished.

These four years in the wilds of California were glorious years for Brewer. Here he had opportunity to utilize all his accumulated knowledge of geology, botany, geography, etc. Things new and unusual were all about him, exciting his curiosity and his admiration; peculiar geological formations, mineral deposits, new and rare plants, lofty mountains, beautiful valleys, wonderful forests, all combined to keep his enthusiasm for the work in hand at a high level. His Journal teems with vivid descriptions of the wonderful scenery of that new country, but his enthusiasm reaches the highest point when he is able to record the finding of some rare plant, such as the Darlingtonia, "the wonder and admiration of botanists," which he discovered near the Castle Mountains, some twenty-five miles from Mt. Shasta.

Equally enthusiastic was he over Mt. Shasta, about which

little was known. Even the height of the mountain had not been definitely determined. Brewer, with Professor Whitney and two others, made the ascent in September, 1862, one of the first parties to reach the top, the highest point of land in California. In his Journal of that date is the following record: "Williamson and Fremont guessed that it (Mt. Shasta) was 17,000 feet high, and so it went into maps at that height. Last year a Mr. Moses measured it with a barometer, which was not graduated low enough, and gave it as a little less than 14,000 feet. So much was known of its height, but of its geology I am not aware that anything was known. We have not found a particle of light in any written authority. No wonder then that we had for long months looked forward to this trip, that we gazed on the mysterious peak with longing eyes from afar off, and that the excitement increased inversely as the square of the distance diminished. We were now camped at its base." He continues, "the first four miles were up a rather gentle slope, through a forest whose beauty cannot be appreciated from any description, mostly of cone-bearing trees, all of species peculiar to the Pacific slope; cedars 20 feet in circumference, spruce as large in girth and taller, sugar pines 4 to 8 feet in diameter and 200 to 250 feet in height, pitch pines nearly as large. I measured one sugar pine that lay beside the trail; it was 7 feet in diameter, the top had been consumed by fire all above where it was 8 inches in diameter, but 225 feet remained—it must have been at least 250 or 275 feet high—and I doubt not that some of the trees of this region are over 300 feet high."

That day they ascended slowly, making various observations on the way and camped for the night at an elevation of 4,100 feet, with the lofty peak of the mountain towering above them. The next morning, many hours before sunrise, they started for the top. Brewer's Journal record reads as follows: "For half a mile or more our way lies over loose blocks of lava, or dry ashy soil, then we strike a strip of snow, up which we follow. But what a path and what a grade. Hour after hour we toil on it, sometimes in the snow, sometimes on a strip of loose rock beside it. The snow is melted into rough waves, often

several feet deep, by the summer's sun, but is now frozen crisp. The bright moonlight gives way to gray dawn, and this to twilight, then the sun comes up and gilds the summit ahead of us, and casts dark shadows far into the valleys beneath.

"We are following up a sort of wide ravine in which the snow lies; on either side are sharp ridges, the naked lava standing out. It appears to have been wreathed in curious forms as it flowed there, and then in later times had weathered into fantastic shapes. Walls, battlements, pinnacles, shooting up hundreds of feet. The ascent grows steeper and steeper as we ascend toward the "Red Bluffs," a sharp ridge at the altitude of almost 13,000 feet. . . . The next thousand feet is less steep but scarcely less difficult, a part of the way over hard lava, or a conglomerate made of pieces of lava cemented with a red volcanic ash, sometimes on snow which was much more difficult. It had melted, forming a surface entirely unlike anything I have ever seen in Switzerland or Tyrol."

The height of Mt. Shasta, as they determined it, was 14,400 feet, "the highest land yet measured in the United States." In his Journal, Brewer wrote, "I feel proud that I took the first accurate barometrical observations to measure the highest point over which the stars and stripes hold jurisdiction." Mt. Shasta was plainly an extinct volcano. As Brewer wrote in his Journal, "geologically, it is nearly as barren as it is botanically. A great cone of lava, little else, not—like Ætna—made up of an almost infinite number of small lava streams, but it seems to have been formed in a comparatively short period by a few gigantic eruptions. It appears to belong to a series or chain of volcanoes that probably had their greatest activity during the Tertiary period, but extended down to comparatively modern times."

Brewer naturally expected to find on the upper slopes of Shasta alpine plants, grasses, mosses, etc., but instead he found utter barrenness. To quote from his Journal, "a few shrubs and sub-alpine plants flourish just above the timber for a few hundred feet, but all above 10,000 feet is a scene of unmixed desolation, not relieved by any plant, nor did we see insect or bird above this height. This barren scene succeeds almost im-

mediately the disappearance of the upper zone of timber, and above 10,000 feet I sought in vain either herb or shrub in the soil, or even a moss or lichen in the rocks. Yet, strange enough, we found the famous 'red snow' in quantities (probably the *Protococcus nivalis*)."

The difference between Mt. Shasta and the lofty mountains of Switzerland, for example, Brewer thought was due to the porous character of the lava composing Shasta. As he wrote in his Journal, "In Switzerland even the few hours of sun on some days, warm into life plants wherever they can obtain soil, up to highest points of naked rock. Springs gush from many points and innumerable rills course over the higher slopes. Not so here at Shasta. The water from the melting snow is immediately absorbed by the porous lava. The soil, a mixture of volcanic ashes and comminuted lava, would undoubtedly possess great fertility with water, but as it is, is dry and dusty. From the summit to the base I saw but one spring, the waters of that sank in a few rods, and one small rill ran from the snows of a ravine, and that, too, sank in less than half a mile from the snow."

With all his work and tiresome travel, Brewer found time to write innumerable letters home, and a perusal of these shows perhaps more clearly than anything else his great enthusiasm for and his deep interest in the natural beauties of California. As geologist and botanist he had enthusiasm and interest in all that the State had to offer to the scienific explorer, but beyond this was a love and admiration of the wonderful scenery all about him; the light and shade on lofty mountains, the brilliant coloring of the flowers in green valleys, the songs of the birds, the magnificent trees in the forests, and the tumbling waters of the mountain streams all called forth a feeling of wonder and admiration akin to reverence, to which he was constantly giving expression.

Brewer was greatly interested in the country along the edge of the San Joaquin and Sacramento valleys, especially the tableland to the north, with its hills of lava and mountain chains. In his diary he writes, "Lassens Peak, and in fact that whole part of the chain, like Mt. Shasta, is a gigantic extinct volcano,

perhaps about 12,000 feet high, a volcano not only much higher but vastly greater in every respect of magnitude and effects, than Ætna, but it is flanked by a considerable number of smaller cones, old volcanoes, from 1,000 feet high, up to that of the main peak itself, many of these cones being much higher and greater than Mt. Vesuvius.

"Here, in a former age of the world, was a scene of volcanic activity vastly surpassing anything existing now on the earth. The materials from these activities not only formed the mountains themselves, and covered the foothills, but also came down on the plain for more than 100 miles. Sometimes volcanic ashes covered the whole region many feet thick, then sheets of molten lava would flow over it, hardening into the hardest rock, then ashes and lava again. These formed beds of enormous thickness, regularly stratified, descending with a gentle slope toward the Sacramento River, and even crossing it in one place near Red Bluffs. But all volcanic action ceased ages ago, and the snows and rains falling on the high lands about Lassens Peak form streams which radiate over it, and they have worn deep canyons, channels in this lava often a thousand feet deep, but generally less. Between these are table-lands, sometimes strewn with loose boulders of lava, othertimes with a surface of nearly naked lava, with only enough soil to support here and there a few straggling shrubs and herbs during the wet parts of the year."

The big trees of California naturally interested him and he measured and recorded the circumference and height of many of the larger specimens, especially those in the celebrated Calaveras Grove. At a height of three feet from the ground, the "Pride of the Forest" had a circumference of 60 feet; "Pioneers' Cabin," 74 feet, "Mother and Son," 82 feet. He recounts how Professor Whitney counted the annual rings of one tree having a diameter of 24 feet inside the bark, cut six feet from the ground, and found its age to be 1,255 years. Many of these trees were 300 feet high, some reaching even 400 feet. Of course, this is an old story now and the "Big Trees" are known to everyone, but in 1863 it was quite different, and Brewer's interest and enthusiasm were fired by these won-

derful specimens of tree growth. All through his life forestry had a great attraction for him, and there is no doubt that these big trees of California, the various groves of which he visited in due time, had a distinct influence in molding his opinions regarding forest conservation and forest management, both of which he advocated strongly later on in life.

During these years of exploration, the party was skimming the cream from the geology and geography of California, passing from one section of the State to another, climbing mountains, mapping valleys, measuring waterfalls, collecting plants and minerals. The labor was hard, but they were keyed up to a joyous endeavor that overcame all obstacles, and to Brewer, especially, no labor was too great when it brought to light some new feature in the geology of the region, some new plant, or a new mineral, or even a new experience in the picturesque or romantic. In the fall of 1863 Clarence King, who had just graduated from the Sheffield Scientific School, joined Brewer and went with him on many of the excursions made the latter part of that year. On one occasion, the last of September, Brewer and King made the ascent of Lassens Peak, about 11,000 feet in height, the description of which Brewer wrote in his Journal, "has the merit of rigid truthfulness in every particular."

"We were up at half past one, had an early breakfast by the light of the bright moon, and at 2:45 we were on our way. First, up a canyon for a thousand feet, then among rocks and over snow, crisp in the cold air, glittering in the bright moonlight. At 4, we are on the last slope, a steep ridge, now on loose boulders and sliding gravel, now on firmer footing. We avoid the snow slopes; they are too steep to climb without cutting our way by steps. We are on the south side of the peak, and the vast region in the southeast lies dim in the soft light of the moon, valleys asleep in beds of vapors, mountains dark and shadowy. At 4:30 appears the first faint line of red in the east, which gradually widens and becomes a lurid arch as we toil up the last steep slope.

"We reach the first summit and the northern scene comes into view. The snows of Mt. Shasta are still indistinct in the

dusky dawn. We cross a snow field, climb up boulders and are soon on the highest pinnacles of rock. It is still, cold and intensely clear. The arch of dawn rises and spreads along the distant eastern horizon. Its rosy light gilds the cone of red cinders across the crater from where we are, Mt. Shasta comes out clear and well defined. The gray twilight bathing the dark mountains below grows warmer and lighter. The moon and stars fade, the shadowy mountains rapidly assume distinct shapes, and day comes on apace.

"As we gaze in rapture the sun comes on the scene, and as it rises, its disc flattened by the atmospheric refraction, it gilds the peaks one after another, and at this moment the field of view is wider than at any time later in the day. The Marysville Buttes rise from the vapory plain, islands in a distant ocean of smoke, while far beyond appear the dim outlines of Mt. Diablo and Mt. Hamilton, the latter 240 miles distant. North of the Bay of San Francisco, the coast ranges are clear and distinct, from Napa north to the Salmon Mountains, near the Klamath River. Mt. Helena, Mt. Johns, Yollobolly, and all the other prominent peaks are in distinct view, but rising high above all is the conical shadow of the peak we are on, projected in the air, a distinct form of cobalt blue on a ground of lighter haze—a gigantic spectral mountain, projected so high in the air that it seems far higher than the original mountain itself—but as the sun rises, it sinks into the valley, and like a ghost it fades away at the sight of the sun.

"The snow on the Salmon Mountains glitters in the morning sun, a hundred miles distant, but the great feature is the sublime form of Mt. Shasta towering above the neighboring mountains, truly a monarch of the hills. In the east, valley and mountain chain alternate until all beyond becomes indistinct in the blue distance. The peaks about Pyramid Lake are plainly seen, while Honey Lake glistens in the morning sun, seemingly quite near. A few miles to the north are four volcanic cones, the highest above 9,000 feet entirely destitute of all vegetation, scorched and broken. The lava tables beneath are covered with dark pine forests, here and there furrowed into deep canyons, or rising into mountains with pretty valleys

hidden between. Several lower peaks about us are spotted with fields of snow, still clean and white, sometimes of rose color with the red microscopic plant, as in the Arctic regions. Here and there from the dark forest of pines that forms the carpet of the hills curls the smoke from some hunters' camp or Indians' fire."

These quotations show more clearly than any words of the writer could how strongly Brewer was impressed by the matchless scenery of the country which he was exploring. They also serve to show the artistic nature of the man, for like a true artist he was seemingly compelled by the very beauty of the surroundings to make a word picture in order to preserve the impression made upon his mind. When we recall the extent of his journeying through California, it is easy to understand that he must have had many occasions for expressing his enthusiastic admiration. In his field work during the year 1863 he traveled 4,243 miles, with a total for the three years of 13,507 miles, of which 6,560 miles were on horseback or mule, and 2,772 miles on foot. Truly he knew California as few other men did.

Not far from Esmeralda the party found traces of enormous glaciers there in earlier times, said to be the first found on the Pacific slope. On the lofty mountains of this region, at an altitude of about 10,000 feet, the alpine plants were especially conspicuous and Brewer reports in his Journal the collection of over one hundred species of mountain plants during his exploration of these mountains. Equally noticeable was the prevalence of *Pinus contorta*, a scrubby pine, at 8,000 to 9,000 feet, this being practically the only tree at a height of 9,700 feet. At higher levels, up to 11,000 feet or more, a low shrub, *Pinus flexilis*, was conspicuous. One of these lofty mountains was named Mt. Dana in honor of Professor James D. Dana, while another mountain, over 13,000 feet high, which Brewer did not succeed in climbing, being stopped 125 feet from the top by the glassy surface of the almost vertical rock, was named Mt. Tyndall, in honor of Professor John Tyndall, the eminent English physicist and mountain climber.

During the winter months in San Francisco, Brewer found time to participate in some of the scientific activities of this city.

He became a member of the California Academy of Sciences and for a time was its recording secretary; he lectured occasionally, taking such topics as the mountain scenery of California, and in many other ways became more or less identified with the life of the place. In 1863 he was elected Professor of Natural Science in the College of California, now the University of California, which position he accepted subject to his duties in connection with the Survey. This position, however, he did not hold long, for in the summer of 1864 he resigned to accept the Chair of Agriculture in the Sheffield Scientific School at Yale. His field work on the Survey ceased in December, 1864, after four years of fruitful endeavor and adventurous life. His experiences during this period undoubtedly had a strong influence on his later career, especially on his instruction in physical geography and in forestry, while his love for California and interest in its development lasted throughout his life. In his bibliography will be found the titles of many papers dealing with observations made during his California experiences, testifying to his continued thought of the phenomena observed there.

Before entering on his duties at New Haven, it was necessary for Brewer to complete his obligations to the Survey. The large botanical collection had to be carefully studied and the plants classified and arranged; new species described and the results put in proper shape for publication. To do all this satisfactorily, proper equipment and expert advice were needed, and these he found in the Herbarium of Harvard University where he worked from December, 1864, until April, 1865, aided by the advice of Professor Asa Gray, who described many of the new species found. Thus was prepared for the press the "Flora of California," or a systematic description of the plants growing in that State, mainly collected by Brewer, including about 2,000 species. The material thus elaborated was finally published in 1876, making a large part of Volume 1 of the Geological Survey of California, under the title "Polypetalæ," by W. H. Brewer and Sereno Watson, the latter having added somewhat to the original collections made by Brewer.

Brewer's knowledge of the western country was further

broadened by a camping trip through the Rocky Mountains in the summer of 1869, with Professor J. D. Whitney and a group of four Harvard students in geology and mining, among whom was William M. Davis. In this exploring trip of three months, Brewer saw much of the geography and geology of the Rocky Mountains, also adding to his botanical collections.

In the spring of 1865 Brewer took up his work as Norton Professor of Agriculture in the Sheffield Scientific School, and for a period of thirty-eight years, until his retirement from active service in 1903, his life was devoted primarily to the interests of this vigorous department of Yale University. The chair he occupied was established as a result of the Scientific School becoming the recipient of the Land Grant Fund of Connecticut, designed by Congress (1862) to promote instruction in agriculture and the mechanic arts. In conjunction with Professors S. W. Johnson, A. E. Verrill, and Daniel C. Eaton, he organized a well-defined course in agriculture which for many years was under the special jurisdiction of Brewer and Johnson, the latter holding the Chair of Agricultural Chemistry. In the history of agriculture in Connecticut the names of Brewer and Johnson are inseparably connected. They worked together for a common good, though by methods radically different, for they were men of totally different types.

As has been well said, Brewer "was a professor of agriculture not only in the Sheffield School but throughout the State," and he labored incessantly for the advancement of agriculture in Connecticut by every means at his command. His wide knowledge and broad experience accumulated through the previous years rendered him peculiarly fitted to bring aid to the farmer. For with all his academic training he retained some of the atmosphere of the soil. He had been brought up on the farm; as a young man he had tilled the soil and gathered the crops. He knew the mind of the practical farmer and he could talk to him in ways that could be understood, and above all he carried with him an air of authority that impressed his hearers and gained their confidence. The farmers of the State soon realized that he was their friend, and that he was actuated solely by a desire to aid them, consequently he was able to

accomplish much for the betterment of agriculture in Connecticut.

His influence, however, extended far beyond the confines of the State. Commencing with the first annual report of the Connecticut Board of Agriculture in 1867, his addresses to the farmers and his many papers on matters relating to agriculture were conspicuous features of the reports during a period of thirty years, adding much of value and thereby helping the circulation of the reports outside the State. The range of his contributions was large, embracing topics quite divergent, but all relating to matters in which the farmer had a live interest, such as the origin and constitution of soils; causes which affect the vitality of seeds; woods and woodlands; practical suggestions on tree-planting in sanitary effects; pollution of streams; the water supply and drainage of farm, house, and farm buildings in their sanitary relations; the educational influence of the farm; the carrying of farm products; the farm in its relation to public health; the English race horse, a lesson in the history of the art of breeding, etc.

While his colleague, Professor S. W. Johnson, was the prime mover for the creation of an agricultural experiment station in Connecticut, the first one to be established in the United States, Brewer was active in support of the movement and was a member of the Board of Control for a period of thirty-three years, serving as a member of the Executive Committee, and its secretary and treasurer from the date of its organization in 1877. Here he rendered valuable service through his knowledge, common sense, and honesty of purpose. These attributes, soon recognized at home and abroad, led to repeated calls for his services on many matters connected directly or indirectly with agriculture. Thus, as special agent on the Tenth Census, he prepared a voluminous report on the cereal production of the United States, in which he studied especially the distribution of production in accordance with geographical, physical, and climatic features; the physical and chemical character of the different cereals; the relation of cereal production to livestock growing, etc. In the Sheffield Chemical Laboratory, under his direction, a large amount of work was done bearing

on the chemical composition of the different grains and their products. He also prepared as a part of this report a brief history of American agriculture. Likewise a report on pasture and forage plants.

When in 1882 the Commissioner of Internal Revenue turned to the National Academy of Sciences for an investigation of the various products formed from starch, notably glucose, a committee of five members of the Academy was appointed to consider this question, Brewer being one of the committee. The report, which was exhaustive and based on a large amount of original work, showed conclusively that the manufacture of sugar from starch is valuable and commercially important: that the glucose is of exceptional purity and in no way inferior to cane sugar in healthfulness. Again, the National Academy was called upon by the Commissioner of Agriculture for an investigation of the sorghum sugar industry, and of the committee of four to study this question Brewer was one.

Brewer was greatly interested in the problems of animal breeding and for many years he gave a course of lectures to his students on the "Laws of Heredity and Principles of Breeding," the syllabus of these lectures having a wide circulation. The evolution of breeds of domestic animals, as illustrated in swine, was a favorite topic and was the subject of many lectures to farmers and State Boards of Agriculture. His study of the development of the American trotting horse, however, was his most important contribution to this general subject. Recognizing that the breeding registers and turf records constitute a great collection of valuable data bearing on the evolution of speed in the horse, he proceeded to arrange and study this voluminous material with a view to determining what the ultimate speed limit of the trotting horse must be. As is well recognized, the origin of most breeds is shrouded in more or less obscurity; it is uncertain how far the special traits are the result of conscious or unconscious selection, what part training, nutrition, and physical environment play in the development of the special qualities. In the case of the trotting horse, Brewer pointed out, "the formation of this new breed is so recent, the development of a special quality has been so marked,

there is such an abundant literature pertaining to its history . . . that we have the data for a reasonably accurate determination of the influences at work which led to this new breed being made, the materials of which it is made, and the rate of progress of the special evolution."

The timing of horses on the race track, as far back as 1806, led to the accumulation of data bearing on the speed of trotters, and about 1818, through judicious breeding and careful training, horses were produced capable of trotting a mile in 3 minutes. By 1830 the speed had been increased to a mile in 2:32 minutes, while by 1881 it had reached a mile in 2:10¼ minutes. Brewer carefully tabulated all the available data, thereby gaining information regarding the rate of increase in speed, etc. Thus, in 1843, there was only one horse having a record of 2:30, while in 1882, 1,684 horses had a record of 2:30 or better. In 1871 there was only one horse with a record of 2:17, but in 1882 there were 18 horses having a record of 2:17 or better. Brewer obviously could not determine how far this fast gait acquired by the well-bred and trained trotter is due to inherited habit, inherited training or to adventitious variation and selection, but he got together a mass of material from which it was possible to plot curves showing how fast horses will ultimately trot and when this maximum will be reached.

In 1873, when the writer first knew Brewer, he was intensely interested in the controversies going on, especially between Bastian and Tyndall, regarding spontaneous generation. Tyndall's work on the dust of the air and the probable relation of the latter to putrefaction, infection, etc., he had followed closely and he quickly realized the full significance of these and kindred observations in their bearing on infectious diseases. He made many observations himself on micro-organisms and taught a small class of students, working under his direction on microscopic technology, the bearings of the newer knowledge on sanitation, water supply, sewage disposal, etc. It was this knowledge, and his faith in the practical value of the results gradually accumulating, that caused him to take an active part in the organization of a State Board of Health for Connecticut, and a local Board of Health for New Haven. This was a form

of public service for which he was peculiarly fitted and he entered into it with all the ardor of a crusader. Opposition of all sorts had to be met and overcome, the people had to be educated, made to see that so-called obnoxious rules and regulations were for their own good. Brewer and his associates had a difficult situation to deal with, but with a combination of wisdom and good sense he guided the growth of these two boards until public health work in Connecticut came to be recognized on all sides as a safeguard to the community. He served as president of the City Board from 1876 to 1889, and as president of the State Board of Health for sixteen years, but he was on the latter board for thirty-one years, i. e., from its organization in 1877 until his retirement on account of failing health. Many-sided knowledge, combined with his pleasing personality, and especially the power to use the spoken word convincingly, yet prudently, enabled him to carry through successfully many plans for the betterment of public health. He was likewise active in the American Public Health Association.

From his earliest years Brewer had manifested great interest in forestry, an interest that had grown steadily with his increasing appreciation of the importance of the forests to the national welfare. His observations abroad, combined with his California experiences, had given him a wealth of knowledge which he was using frequently in support of proper methods of maintenance and increase of the forests of the country. In 1874 he prepared a map, based on the results of the Census of 1870, for Walker's Statistical Atlas, showing the distribution of woodland and forest systems in the United States. The following year he wrote a report which gave an analysis of the forest resources of the country. Later, when public attention was being directed to the declining condition of the forests, he became an ardent advocate for a thorough investigation of the matter, and in 1896, when at the request of the Government the subject was taken up by the National Academy of Sciences, he was one of the Commission appointed to investigate and formulate proper methods for the preservation of the forest resources of the country. As a member of this United States Forestry Commission, Brewer traveled widely over the country to the west-

ern coast and took an active part in the survey which the Commission made. Eventually, as a result of the recommendations submitted to the Government, the National Department of Forestry was established, with Mr. Gifford Pinchot as Chief Forester. When in 1900 the Yale Forest School was established, Brewer took an active part in its organization, serving as a member of its governing board and giving for several years a course of lectures on forest physiography and meteorology. He was likewise active in the organization of an undergraduate course in "Studies preparatory to the study of forestry," in the Sheffield Scientific School.

Following the request of President Roosevelt, the National Academy of Sciences appointed a committee to consider and report upon the desirability of instituting scientific explorations of the Philippine Islands and on the scope proper to such an undertaking. Of this committee Professor Brewer was appointed chairman. As a result of the study made by this committee a comprehensive report was submitted in 1903 recommending that the Government undertake the following scientific explorations: coast and geodetic work and marine hydrography; land topography, including surveys and classification of the public land; geology and mineral resources; botany; problems of forestry; zoology; anthropology. To this study Professor Brewer devoted much thought and effort, and he often expressed the wish that he might go to the islands and view with his own eyes the resources that existed there.

For many years Brewer gave to the students in the Sheffield Scientific School a course of lectures on physical geography. He naturally took great interest in this subject, his broad experience as a geographer in the survey of California, his travels elsewhere, his geographical studies in general, all combined to render him peculiarly fitted for work in this field. With his customary enthusiasm, he brought together much new and original material, built up a large collection of maps, books, and photographs, revised Warren's Physical Geography, making of it, with the new material added, one of the most authoritative textbooks on this subject, and thus created a department of study at Yale that won general recognition. As chairman of the

commission appointed to organize a topographical survey of Connecticut in 1889, he gave during a period of six years freely of his time and knowledge to insure the survey being made with proper accuracy and with due regard to economy. As a result, largely of his efforts, excellent and useful maps of the State were produced. Among the many subjects in geography that especially interested him was the subject of river deposits in their bearing on delta formation. For many years he carried on experiments upon the mechanical suspension of clays in river waters and the conditions under which their sedimentation takes place, studying particularly the effect of small admixtures of mineral salts upon the rate of precipitation.

His interest in geography and geographical research often led him far afield. Twice he went on exploring voyages to the far north; once into the Greenland seas on the steamship *Miranda,* which unfortunately was wrecked near the Arctic Circle, exposing the people on board to grave dangers, from which they were rescued only after a long period of serious discomfort. The second voyage was into Behring Sea in 1899 with the Harriman Alaska Expedition, where he saw much to interest him. In the second volume of the scientific reports of this expedition is a paper by Brewer on "The Alaska Atmosphere." When the Arctic Club was founded, Brewer served for many years as its president.

To give an adequate presentation of all that Professor Brewer did for the Sheffield Scientific School at Yale during his long period of active service—from 1865 to 1903—would be a difficult matter. In the early years, when the very life of the institution was uncertain, Brewer, like his associates Brush, Gilman, Johnson and a few others, gave all his strength and energy to place the school on a firm foundation. Through his broad training he was especially qualified to fill many gaps, and this he did with unfailing generosity and with complete disregard of self-interest. Later, when he was able to limit his activities to those subjects in which he was especially interested, he became through his knowledge, enthusiasm, and patience one of the most beloved instructors in the Scientific School, looked on by his large classes of students with admiration and respect.

In the words of one of his admirers, "he was one of the most striking personalities connected with the Scientific School and with Yale University." As a member of the Governing Board of the School he helped shape its policies for that time and for the future, and as a member of the Board of Sheffield Trustees, he shared with others many of the financial and other responsibilities that pertain to such a board. His conscientious performance of all duties that devolved upon him, his excellent judgment and wise counsels made him invaluable, while his unfailing courtesy, kindliness of heart, and sweetness of disposition rendered him an associate with whom it was a pleasure to work.

His services to science and to the public were widely recognized. Washington and Jefferson College, in 1880, conferred on him the degree of doctor of philosophy. This same year he was elected a member of the National Academy of Sciences. In 1903, at the time of his retirement from active service in the university, Yale gave him the degree of doctor of laws, and the same year Wesleyan University conferred on him a similar degree. In 1909, at the time of the celebration of its fiftieth anniversary, the University of California gave him the degree of doctor of laws.

On August 14, 1858, he married Angelina Jameson, of Ovid, New York. She died in June, 1859, and ten years later, on September 1, 1868, he married Georgiana Robinson, of Exeter, New Hampshire. Of this marriage there were four children, viz., three sons and one daughter.

During the last two years of his life, failing health curtailed his activities and imposed restrictions more or less irksome, but these Brewer accepted with characteristic calmness and philosophy. On November 2, 1910, he passed quietly away, eighty-two years of age.

Brewer's traits and achievements were in a measure the result of what he had derived from John P. Norton, Bunsen, and Liebig. From them he acquired a stimulating eagerness for research which led him in many directions, into many fields. He was essentially an explorer, carefully recording his observations on every sort of scientific subject. There were no limita-

tions in his search for truth, for with his observing temperament he could not be content in any narrow field of research, no matter how much of interest it might have for him. He was led in many directions not through lack of thoroughness, but partly because the many services demanded of him he regarded as duties, and partly because of his broad interests and his love of scientific adventure. One is reminded of the words of Sterne: "What a large volume of adventures may be grasped within this little span of life by him who interests his heart in everything and who, having eyes to see what time and chance are perpetually holding out to him as he journeyeth on his way, misses nothing he can *fairly* lay his hands on." Brewer never wittingly missed an opportunity to see and record; his powers of acquisition were exceedingly great, but he lacked the disposition, or rather the opportunity, to digest all that he had absorbed.

Again, he was so deeply possessed by a sense of personal obligation to the community that he often sacrificed himself and his time for the benefit of others in a way that few men are willing to do. As one of his colleagues wrote: "His knowledge, suggestiveness, and original ideas were at the service, and freely given, alike to his friends and to the stranger who sought him, and innumerable must be those who profited by them. How far what has been accomplished for general advancement, at many times, in many places, and by many people through his help and influence cannot be measured, but the sum total would be astonishing if we could but know it." Public service was the keynote of his life; a life of unselfish effort, in which his knowledge and experience were freely given.

Apart from his professional work and scientific achievements there is another side to Brewer's character that calls for comment, if there is to be a judgment of the whole man. His social qualities were of a rare order. He was endowed with conversational gifts of an unusual quality, and as he was possessed of a memory retentive to a remarkable degree, with a broad experience of life under many conditions, he was a delightful companion, having a fund of stories, anecdotes and merry jests that enlivened any group of which he happened to be a mem-

ber. He radiated good nature, enjoyed wholesome fun, and was quick with an apt quotation or witty rejoinder when in congenial company. To those who knew him intimately his genial fellowship and friendly, generous nature constitute an abiding memory.

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